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THE PRACTITIONER:

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A JOURNAL

OF

THERAPEUTICS AND PUBLIC HEALTH.

EDITED BY

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THE PRACTITIONER.

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Original Communications.

THE LATE DR. ANSTIE.

BY THOMAS BUZZARD, M.D.

It was in October 1853 that I became acquainted with Francis Edmund Anstie, who had then just entered the Medical Department of King's College, in which I had been studying for two years—and my memory pictures him as a tall and very thin young man, with an earnest good-looking face, and a remarkable eagerness of manner when his attention was excited. As a student he was steady and diligent, without however displaying any extraordinary force of originality in the profession of his choice. Although he had no complaint to make of his health, his spare frame and somewhat hollow features seemed to imply a delicacy of constitution the influence of which appeared at that time, in some measure, perhaps, in his personal characteristics. He was inclined not exactly to lounge, for that would suggest idleness, an idea which could never in any part of his career be justly associated with him. But he was not indisposed to stray from the clearly-defined path of a particular study—to turn up, as it were, some byway of thought in company with a sympathetic friend, and discuss points of interest the bearing of which upon his work as a medical student was not quite evident. He was not long in becoming popular. Bright, good-natured, possessed of a keen sense of humour and a capacity for saying quaint things which were always without a spark of malice, with a correct ear for music, and a tenor voice which was of quite exceptional quality, it is not wonderful that he took with his fellow-students. I can remember that he was generally called "old"—always a sign of attachment amongst youngsters; and that there was a kind of laughing tone adopted in speaking of and even to him—a tone which was not exactly that of banter, and was not quite free from a trace of patronage. If my idea be right, this was to be explained in the main by one particular quality of Anstie's mind—he was singularly absent. In after life this absence became somewhat less marked; perhaps, because in the multitudinous claims upon his attention it was more easily excused; but as a student it was a notable characteristic, and would often give rise to merriment, which, as is always the case when it is excited by a weak point in a fellow-creature, was slightly patronising in character, or at least was marked by an unconscious tinge of superiority. In this case, however, it was quite as strongly coloured by affection. "Old Anstie's" slips of memory leading to queer results, would create at times a shout of laughter, but the laughers would be drawn nearer than ever to one whose good temper caused him to enjoy the joke as much as anyone—and whose manifest ability and force of character always preserved him from any chance of occupying a really undignified position.

Such is the first impression which I gained of Anstie. It was one which was destined to be modified in the course of time more than perhaps is usual, for notoriously men are apt to grow up with singularly slight alterations of character. It was long after our acquaintance as students that I learnt anything of his previous life, and what I have gathered does not amount to much: that little may best be told here.

FRANCIS EDMUND ANSTIE was the youngest child of a manufacturer whose family had been settled in Devizes for many generations. He was born in that town on December 11, 1833. His father, the late Mr. Paul Anstie, a man of great force of character, energy, and public spirit, was one of four brothers who were all somewhat exceptionally remarkable for strong political convictions. As sturdy Liberals they had fought many a hard battle in times when political feeling ran very much higher than it is easy in these days to realize—now that from thirty to fifty years have passed away. The family was nonconformist, and Francis was brought up in the views of his ancestors. In the matter, however, of religious opinion, as indeed in all others, he showed when he reached manhood a determination to think for himself; and, to anticipate a little, it may be here said that as the result of much and solemn consideration he made his decision, and at the age of about twenty-six was admitted a member of the Church of England. His scholastic education was carried out entirely in a small school kept by Dr. Biggs, at Devizes. Some eight or ten boarders and half a dozen day scholars, of whom Anstie was one, composed a circle which was so small that the instruction easily assumed a personal and tutorial character. Dr. Biggs, I am told, was a man of very high mind and powerful influence, with a strong bias towards ratiocination. His especial point in teaching was to make his pupils thoroughly understand every step of their way, and to this he would apply himself with never ceasing energy and deserved success. Anstie went to this school when he was between nine and ten years of age, and remained there until he was sixteen. He was very tall for his years, manly, and bold. A peculiarity at this school was a system of purchasing holidays—the purchase money consisting of tickets obtained for some success in study. The holidays, too, were spent in original fashion. A waggon was hired, and the master and pupils would pervade the country in excursions which were full of delight to a boy like Anstie, who was always fond of novelty and adventure. Those who knew him at this time describe a certain abruptness in his manner, a tendency to idealism, "an intellect which was more marked in its synthetical than its analytical powers," and they say that he had a lively imagination and a singularly affectionate disposition. He was never a leader at school. His cousin, Mr. James Anstie, of the Inner Temple, Barrister-at-law, who was with him at school, has been kind enough to give me some particulars of his school life. He had, it appears, like other boys, his fits of idleness, but on the whole worked steadily. He was quick at languages, and read his classics with eager relish; but, always ready to seize an idea, he was more apt to be rapid than precise. In the playground he was eager and impetuous, and played every game with life and spirit. His temper was perhaps quick, but his good nature always loved to avoid a quarrel and never allowed him to persevere in one, and his free-handed generosity made him always popular. His lively fancy, and buoyant spirits, and energetic self-confidence often gave a tinge of exaggeration both to his words and acts; but bombast and pretentiousness were then, as always, foreign to his nature. A wise indulgence on the part of his parents, and the greater freedom of a day scholar's life, which kept school life and social life in a mingled current, accorded happily with his natural disposition, gave variety to his interests, and fostered his early development. Though in a small country town no very powerful intellectual current can be looked for, Devizes had at that time its full share of intelligent activity, and as the vivacity of his mind and the self-reliance of his character made his society welcome to those much older than himself and to men of very various character, his rather long residence there was far from being an unfavourable preparation to his subsequent career. He came to town, not only a well read man, but with a knowledge of men and things certainly superior to that of most of the same age who had been bred in cities.

His bent for music was early remarkable, and it would seem that he picked up for himself a knowledge of the science which raised him in after life far above the ordinary level of amateurs; for he does not appear to have had any regular instruction in this subject. He is described as having "always had" a faculty for reading music and singing, and the atmosphere at Devizes in those days was distinctly musical.

In 1850 he was articled to his cousin, Mr. Thomas Anstie, a medical practitioner of Devizes, and with him he remained until he entered the Medical Department of King's College in 1853. As an apprentice he passed no little of his time in devouring general literature. His brother-in-law, the Rev. Charles Stanford, was his frequent companion during the six years which preceded his departure for London. "It was with me," he writes, "that Frank had his first eager talks about books, when the worlds they open were all delightfully new to him. He was glad to join in discussions about antiquities, things that light up history, biblical questions, and subjects that book-hunters take pleasure in." "During this pupil life at Devizes," I learn from the same authority, "he seemed to learn a great deal accidentally and without much hard work. At that time there was but little development of that 'transcendent capacity for taking trouble' which, according to Carlyle, is genius. Apparently, too, there was no strong bias in the direction of science. Indeed, his passionate love of music and beautiful voice, his keen sense of 'the comedy of life,' and the gift of charm that he possessed—suggested to some a fear that he might be drawn into society to a degree that would interfere with steady labour and great success."

The Medical Department of King's College held a remarkable place in the educational institutions of this country some twenty years ago. Possessing what was then probably, so far as regarded its construction, the most imperfect general hospital in London—it was simply the tottering remains of the workhouse of St. Clement Dane's, which looked upon a disused graveyard—the ability of its professional staff had sufficed to overcome this as well as certain other disadvantages connected with the institution, and to attract such a gathering of young men from all parts of the kingdom as made it at that time one of the largest and most esteemed of English medical schools. The skill with which Fergusson was wielding the knife, the fame of Partridge as a graphic teacher of anatomy, the justly-earned and enduring reputation of George Budd as a pathologist, the rapidly-growing estimation in which Bowman was being held as a physiologist and ophthalmologist, combined towards this powerful attractiveness. But more than all, perhaps, it was to the genius and power of fascination possessed by the late Robert Bentley Todd that King's College was indebted for its influential position among the metropolitan schools of medicine. In the opposition with which great reformers are always met, exaggeration and misrepresentation constantly find a large place. In the case of the late Dr. Todd this was very marked. Ridicule was freely showered upon his use of alcohol in the treatment of acute disease, and both in his lifetime and since his death he has been very widely accused of a systematic abuse in the quantity of stimulants prescribed by him. This is not the place in which to enter into a minute discussion of the degree of truth contained in the charges which have been made against him, but a few words on this point will not be out of place. There is no doubt whatever that the allegations were often full of the grossest exaggerations based upon an ignorant prejudice. He suffered, too, as not uncommonly happens, still more severely from his friends. Captivated by the idea of novelty, shirking the trouble of watching Todd's practice or seeking to understand his views, there were not a few of those who dubbed themselves his followers who quite mistook his principles, and went to the absurd length of investing brandy with the character of a specific against almost every disease.

It must be remembered that Todd was, so far at least as England was concerned, the apostle of a new doctrine, the germs of which he had received from his revered master, Graves of Dublin, who had adopted so long previously as 1833 the use of stimulants in the treatment of fever. Whilst Todd was teaching at King's College that the patient, not the disease, was the proper object of treatment, and fighting against the prevalent error which, on vitalistic notions not based on experience, ascribed to alcohol the quality of increasing inflammatory processes, teachers of medicine at the other metropolitan schools were impressing upon their class that the first question to be asked in the consideration of therapeutic aid was, were we to bleed? Alcohol he prescribed with an unsparing hand in inflammatory diseases, because he looked upon it, in proper doses, as food, and had found that any evil arising from accidental excess did not lie in the direction of increased inflammation. He showed that in all cases of brain affection from alcohol, the symptoms were due, not to congestion nor to inflammation, but to a simple poisoning of the nerve-fibre and nerve-cell by the spirit. The same effect was caused by chloroform and ether when inhaled. They acted directly upon the nerve-fibre and nerve-cell, and did not produce congestion.

Combating the idea of certain objectors, that if alcohol causes exaltation of nerve force, this is followed by a corresponding amount of depression, Todd insisted that this was an error due to a comparison of a patient's feelings when under the immediate influence of the spirit, with the sensations which accompany the absence and want of its exciting influence. There was no evidence to show, he taught, that any physical depression is induced as a sequel to the alcoholic excitement, that any increased waste goes on, or that the nervous power is at a lower ebb than before the administration of the alcohol, if it have been given in proper quantity. He thought that when alcohol had been taken in inordinate quantities, then the feeling of depression was the greater by reason of disturbed digestion. Alcohol, he taught, when taken carefully and under proper circumstances, increased the animal temperature, strengthened the action of the heart, and reduced the frequency of the pulse. It was a form of aliment appropriate to the direct nourishment of the nervous system. In his directions for the administration of alcohol,[1] he said : "You must give enough to keep up animal heat, and to protect the tissues without embarrassing the stomach, and without allowing the fumes of alcohol to be perceived in the breath. All that is given over and above that quantity is wasted, so far as the support of the vital powers is concerned." As it was only by trial that the wants of the system could be measured and the quantity adjusted, he inculcated the plan of giving a precise and at first small dose, freely diluted, at brief intervals. He protested against the imputation of encouraging the excessive and improper use of alcohol. That alcohol was sometimes administered by Todd in cases which did not require it, and in quantities which were excessive for the need, is, I suppose, hardly to be doubted, but it must be remembered that Todd was a pioneer, that he was breaking new ground, and that his method of handling an agent about which at that time but little was known as regards the mode of its disposal in the economy, was necessarily in a high degree tentative. It must be admitted that he based his practice upon a theory which has failed to withstand the test of time. That he was innocent of the charge of a blind recklessness in his application of alcohol is certain. He was inquiring, and modern medicine has reaped the benefit of inquiries which were conducted in a fearless spirit. It is always difficult, and usually impossible, to apportion the degree of originality which rightfully belongs to those who introduce innovations in medical practice, and I will not attempt to decide how much Todd owed to Graves or to any other predecessor in the doctrine of giving support to the patient. In general terms, the fact undoubtedly remains that when Todd was giving stimulants in cases of acute inflammation, his practice was directly opposed to all the teaching of his day.

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[1] "Clinical Lectures on certain Acute Diseases," by R. B. Todd, M.D. London, Churchill, 1860, p. 461.

In order to understand the causes which operated in determining the line of study to which Anstie became committed later on, and to account for the enthusiasm which he threw into his work, it is necessary to glance for a moment at the ideas which had not ceased to be common when he was a student. It was thought that inflammation was the result of an extra amount of strength, operating doubtless in a confined space, and accompanied often by general weakness of the patient; but, so far as the district of the inflammation was concerned, there was supposed to be a morbid accumulation of force—a dangerous potency which it was necessary to subdue by general or local bleeding, purgatives, tartar emetic, and starvation. When the rebel was thus reduced to submission, when the inflammation was "knocked down," as the phrase was, it was thought time to consider about attempting to carry on the ordinary nutrition of the patient's body. From such axioms it seemed to follow that a substance like alcohol must increase the local excitement and add "fuel to the flame." Doubtless some metaphorical words had come to be used as though they expressed a literal truth. Alcohol is inflammable, and, consciously or not, the idea of its power of keeping up the combustion which was supposed to be associated with inflammation was a fixed one in men's minds. It is only just to remember that Todd showed us that we may give wine or brandy in selected cases of acute inflammation without a shadow of misgiving. It was reserved for Anstie to carry forward, and bring to a condition of practical utility, Todd's somewhat crude views as regards the use of alcohol; and the pupil lived to give a precision to its employment which was wanting in the practice of the teacher.

In his researches upon the nervous system as a physiologist and clinical physician, Dr. Todd may fairly be placed amongst the foremost leaders of the modern school, and for no short period he was singularly in advance of the age. The conclusion to which he came, and which he expressed some twenty-five years ago, respecting the co-ordinating function of the posterior columns of the spinal cord—an opinion which extended clinical observation as well as the experimental researches of later investigators has confirmed—was one of the most striking examples of his acumen in this branch of study. His *clinique* at the hospital, always well attended, was especially valued when cases of nerve disorder were the subject of his graphic demonstration. .One will not easily forget the grand head and the silver tongue of him who was then *par excellence* the physiological physician!

This was the master to whose teaching Anstie was soon and powerfully attracted; and the student, who listened with willing ears, was one who had then—and his after life showed that he continued to retain it—a singular disposition to veneration, when the object was a worthy one. I have often been struck with this in him. Anstie, who was so regardless of persons and bold in the expression of his opinions—qualities which sometimes gave him a false air of arrogance—had the faculty of reverence for what was superior in any way, morally or intellectually, intensely developed—more prominent, indeed, than I remember to have seen it in any other man possessed of such characteristics. No wonder, then, that his nature was powerfully impressed by the fascination of Todd's genius, and that the ideas which he imbibed at this source served to colour his whole future life. And this bent had new force added to it when at Todd's death his practice was bitterly attacked, and Anstie's spirit of chivalry roused him to go forth in defence of his master. The action of alcohol and the study of diseases of the nervous system became the subjects which most largely engaged his attention throughout his career. In process of time Anstie held the office of clinical clerk to Dr. Todd, and took full advantage of this opportunity of studying the views of his great teacher. About this period a paper upon "Syphilitic Paralysis," which he had read at the King's College

Medical Society, was published in the "Transactions." This, his first publication, is remarkable for the signs of facility in composition which he showed even at that early stage of his career.

In 1856, Anstie took his diplomas at the Royal College of Surgeons and the Apothecaries' Company. Uncertain as yet in which direction his professional life would tend, he became, in 1857, physician accoucheur's assistant, resident at the hospital. This branch of practice, however, was always distasteful to him. His impulsive and anxious temperament somewhat unfitted him for duties which, more perhaps than any others, require patience and confidence. During his period of residence, his colleagues being Mr. Christopher Heath, who was house surgeon, and Dr. Alfred Meadows, house physician, social gatherings with a strong musical element used to be held in the out-patient department on many Saturday evenings. These meetings, which were attended by numerous past as well as actual students, will never cease to be remembered by those who took part in them, and will always be associated with Anstie's melodious voice and genial presence.

In the autumn of 1857, he took his M.B. degree at the University of London, and was at first for some time occupied in administering chloroform for Sir William Fergusson. He lived for a short time in Stamford Street, and about this time, in order to gain experience, he went to the east of London and assisted a medical man who was largely engaged in practice. He removed to Onslow Square in 1859, and, having proceeded M.D., joined the College of Physicians as member, and was appointed physician to the Chelsea Dispensary. By this time he had determined to practice medicine; and having been appointed pathologist to the Westminster Hospital, he was elected assistant physician and co-lecturer on forensic medicine in 1860, in the vacancy caused by the transference of Dr. Russell Reynolds to University College Hospital. He subsequently exchanged this lectureship for that on Materia Medica, which he held for many years.

During his residence in Onslow Square, his house was the place of meeting of a circle of friends who came together on Saturday evenings for talk and music, and aimed at a high standard of artistic, literary, and scientific conversation. In social gatherings of this kind Anstie was well qualified to take a prominent part, whilst his frank and genial temperament made him powerfully attractive to his associates. Dr. Blandford, then resident medical superintendent at Blacklands House Asylum; Dr. Way, now in practice in Eaton Square; Mr. Whitehorne, of the Chancery bar, were amongst those who formed this circle. Anstie belonged also, at that time, to a musical society which met alternately at the houses of Mr. (now Sir Henry) Cole, C.B., and Mr. Mould.

These references to the lighter side of life should not be misinterpreted. At this period, although he had not as yet entered upon a path of work which was destined later to lead him to exertions which were remarkable, even in the profession to which he belonged, for their laborious character, Anstie was no *dilettante*. He read much, especially in the works of French pathologists and chemists, and applied himself with unsparing energy to the duty of seeing out-patients at the Westminster Hospital. He was a regular attendant at the Western Medical and Surgical Society (now extinct), and contributed several papers at its meetings. His position as a teacher of forensic medicine determined the character of many of his contributions, one of which was "On the Proofs of Live-birth in relation to Prosecutions for Infanticide."[1] He wrote, too, in the medical journals on the subject of chloroform. Between 1854 and 1860 he had administered it on more than 3,000 occasions without meeting with a fatal accident. He always strongly deprecated the practice, then very common, of using a mere napkin, and advocated the employment of an arrangement by which accuracy of dose and dilution might be secured—at first using the inhaler which was devised by Dr. Snow, and later giving his preference to the plan introduced by Mr. Clover.

It was in 1860 that Anstie first came prominently forward as a champion of Todd, who had recently died, and whose practice had been attacked in print. He published a powerful letter,[2] in which he disposed effectually of many misconceptions connected with the views prevailing about the use of alcohol.

In the month of August 1862 Dr. Anstie married the second daughter of Mr. Wass, of The Lea, Cromford, Derbyshire. The pages of a scientific journal are not the place in which it would be proper to dwell with any minuteness upon details of domestic life, but I cannot help recording that this union, which lasted till his death, was one of unbroken happiness. In his wife Anstie found a devoted and sympathetic helpmate, peculiarly fitted to be the companion of one whose quick and impulsive temperament might easily have been misinterpreted by a mind less qualified to discern and appreciate a true nobility of character. The increased responsibilities which marriage brings operated upon Anstie as a powerful incentive to exertion; he began to devote himself more closely than ever to the practical business of his calling.

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[1] *Medical Times and Gazette*, Nov. 16, 1861.

[2] *Medical Times and Gazette*, April 28, 1860.

About the time of which I am writing there was a monthly journal called the *London Medical Review*, which was edited by Dr. Meadows, and contributed to, amongst others, by Anstie, Brudenell Carter, Christopher Heath, George Lawson, Dr. Inman, and the late Dr. Tanner. I think it was in this journal that Anstie wrote his first essay on the subject which engrossed him so much afterwards. The paper was on "The Alcohol Question,"[1] and the author's aim was to refute certain popular theories which it seemed to him were erroneous and embarrassed the general question of the exact value and rank of the substance. As representing the most common idea, the view of the late Professor Miller, of Edinburgh, was quoted:—"Alcohol is a narcotic stimulant, one of a class of substances which, given in repeated small doses, will produce a stimulant effect which may be kept up for some time; an effect, however, which will be certainly followed by a depression, profound in proportion to the length of time during which it has been delusively postponed." He sought to show that this description was inaccurate. It had been made certain by the researches of MM. Lallemand and Duroy that alcohol specially directed itself to the nervous system, its effects varying extremely according to the amount and concentration of the dose, so that there was no analogy between the pleasant excitement produced by a glass of good wine and the dull stupor following a very large dose. It had not been proved that the two kinds of symptoms were part of one and the same physiological sequence. Nothing more was known than that the exaltation of moderate alcoholism and the paralysis of intoxication were due to the respective conditions of the nervous system in the two cases. Universal experience of the effects of a moderate dose (its amount depending upon idiosyncrasy, or temporary condition of system) showed that this was not followed by any after depression. The ingestion of a large dose caused drunkenness, i.e. *palsy* of a partial and special kind. The poisonous effect began with the first symptoms of true inebriation, such as incoherence of ideas, slight thickness of speech, want of power to co-ordinate the movements of the eyes. He quoted from a writer in a journal who had criticised Dr. Lankester's employment of the term "medicinal food" to alcohol, tea, coffee, and certain other substances, these amongst other remarks: "Remedial agents, properly so called, should be regarded as lesser evils to which the administrator has recourse to preserve the body from the influence of greater evils—disease. That all remedies of this kind are positively evils, *i.e.* hurtful to the body in its natural state, is plainly manifest; for, if they were not so, they would not be remedies at all. Remedies produce, and are given to produce, an abnormal action."

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[1] *London Medical Review*, 1862.

I have given this passage at length, both because it illustrates what was still a prevalent notion at the time (only thirteen years ago!), a notion that the disease, not the patient, was the proper subject of treatment, and because it will conveniently introduce Dr. Anstie's comments upon it. He writes: "In order to place the inaccuracy of its statements in the strongest light, it is only necessary to refer for a moment to two medicines which, in this age of curious and sceptical investigation, command the undoubting confidence of physicians in the treatment of a large number of diseased conditions, viz., iron and cod-liver oil. Now, these two remedies most undoubtedly are foods, and act as such, and yet they are most potent medicines, *i.e.* healing agents. And an intelligent observer of the progress, slow as it is, of therapeutics at the present day, would not fail to be struck with the singular and notable fact, that the central idea, the very foundation of all our most modern plans of medicinal treatment, is that of aiding *nutrition,* and the various processes which subserve it. The doctrine that 'all remedies produce, and are given to produce, an abnormal action' is a relic of that past age of medical darkness when the now justly-exploded theory of Allopathy was in vogue—an age which, for its sins of ignorance and lazy theorising, was followed by homœopathy and a hundred other quackeries which have come down upon us." Here was the doctrine, born doubtless of Todd's teaching in the first instance, and more or less strongly supported by some who followed him, Radcliffe, Chambers, and others, which Anstie preached throughout his career with a vigour which ensured its diffusion and procured a large amount of reception for the truth which lay in it. The principles enunciated were those which animated his teaching at Westminster Hospital, and made his lectures on Materia Medica something widely different from the vapid mixture of botanical terminology and extinct vitalistic theories which had hitherto characterised the greater number of such productions.

But to resume my reference to the paper under examination. The author proceeded to point out that it was not merely with regard to medicines like cod-liver oil and steel, which really do become converted into tissues of the body, that one may say that they are, in a true sense, foods; but that whatever substances are able of themselves to maintain life during a considerable period, although at a low standard, are entitled to be called foods. Water would do this, and so would alcohol. He contested the very common idea respecting poisons—viz., that there were a certain number of substances, which could be spoken of as a class distinct from other substances, the action of which upon the human body was uniformly and in all doses deleterious, and instanced common salt, which in small doses was a necessary element of food, whilst in large doses it acted as a violent irritant poison, as one of many substances whose action differed essentially with its dose. Summing up, he suggested—1. That in small doses, not too frequently repeated, alcohol acts as a special stimulant of the nervous system, and exerts no poisonous action on the body whatever. 2. That alcohol in large doses acts as a narcotic poison, and that its operation as such is purely depressing. 3. That alcohol, if it can be shown to possess the power of singly sustaining life, although imperfectly, for any considerable period, ought to be considered as a *food*. He pointed to the fact of a large number of chronic drinkers who were unable to take any solid food, and did practically sustain life for years upon beer and spirits, and quoted examples of patients suffering from acute inflammations who had been supported for long periods upon alcohol alone without suffering marked emaciation. He closed with the following propositions:—1. Alcohol is capable of sustaining life, in the absence of all other foods, for many days. 2. During acute diseases, alcohol is sufficient, without the help of any other food, to prevent emaciation of the body, and also the extreme lowering of the muscular strength, which would render the period of convalescence tedious. 3. Given in acute disease, no amount of alcohol which the exigencies of the case require will cause inebriation; on the contrary, delirium, unless it depends on inflammation of the brain, may be checked by the administration of alcohol. 4. That the demand of the system for alcohol in acute disease is in inverse ratio to the power of assimilating other foods.

In this paper, Anstie confined himself to a small part of the question, "What is the exact rank and value of alcohol?" a part upon which light could be thrown by clinical experience. There were other considerations, however, which at that time were beginning to engage his eager attention. As has been hinted in an earlier part of this paper, Todd had founded his employment of alcohol upon an acceptance of the view, based upon Liebig's theory, that, with the exception of a small portion which escaped unchanged in the breath, alcohol, when in a dose proportionate to the necessity, was evolved in the form of carbonic acid and water, and promoted the secretions of the lungs, the skin, and the kidneys. The union of alcohol with oxygen within the body gave rise, it was supposed, to the generation of heat. But shortly after Todd's death, the appearance of a work by MM. Lallemand, Perrin, and Duroy,[1] threw the gravest doubts upon the correctness of this simple explanation. These writers stated that they had proved by experiments, what had been more or less strongly asserted by others before them, that the alcohol which was taken into the body accumulated in the tissues and was shortly cast out through the various excretions, totally unchanged. There was no union, they said, with oxygen, no combustion, no change therefore into carbonic acid and water, accompanied by evolution of heat. So numerous and apparently precise were the experiments, which had been carried on during two years, and so clear were the indications derived from them, that it seemed to most persons that the matter was definitely settled. Anstie studied the book of the French experimenters, and instinctively felt that, although he could not put his finger on the fallacy, some important error lay in the investigation.

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[1] Du Rôle de l'Alcool et des Anesthésiques dans l'Organisme. Paris, 1860.

It was at Mr. Cole's house, next door to his own, I think, that Dr. Anstie made the acquaintance of the late Mr. Thackeray, who was then editing the *Cornhill Magazine.* In the number of that journal for June 1862, there appeared an anonymous article, entitled, "Is it Food, Medicine, or Poison?" written in a popular style, and discussing the question with singularly graphic power. The article excited a great deal of public interest, and was followed by another in September of the same year, styled, "Is Alcohol Food?" Anstie was the author of these, and in them he shadows forth the doubts which were then arising in his mind in respect of the work of Lallemand and Perrin. After noticing the strong conviction that had taken possession of men's minds in consequence of these experiments, "I do not happen," he says, "to share in the belief that the new oracles have effectually settled the question."

He carried out about the same time an experimental inquiry into the physiological action of podophyllin, the details of which were published in March, 1863.[1] Numerous experiments were performed upon the lower animals, an alcoholic solution of the resin being injected into the peritoneal cavity. The conclusions at which he arrived were, that no irritant action was produced upon the serous membrane unless the resin remained unabsorbed, and lying for some time in contact with it; that the substance passed into the blood, and exercised an irritant influence upon the intestines; that as a secondary result of this irritation, or perhaps as a mere consequence of the squeezing of the gall-bladder by the abdominal muscles in repeated efforts at defecation, bile was occasionally poured out in large quantities, but that this was by no means necessary; that no inflammation of the liver was produced by poisonous doses; that it appeared pretty certain that the substance did not act directly on the liver.

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[1] *Medical Times and Gazette.*

The papers in the *Cornhill Magazine* were so successful that he was encouraged by the editor to supply some more, and in the course of the next few months he contributed several articles in which he dealt in a similar vein with various subjects of physiological interest. In November 1862 there was one on "Tobacco, its Use and Abuse;" in the following March, on "The Medical Evidence of Crime;" in April, on "Corpulence;" in July, on "Over-eating and Under-eating;" in August, on "Medical Etiquette."

He had now resolved to move into a more central part of the metropolis. In Onslow Square he had had but very little practice, and the position which he was taking as a physician rendered a change of quarters desirable. So in May 1863 he moved into the house which Mr. (now Sir Henry) Thompson was quitting, No. 16, Wimpole Street. His forthcoming book upon Stimulants and Narcotics was then engaging him in very severe work, both in laboratory experimentation and in the consultation of authorities. The processes described by Lallemand, Perrin, and Duroy were gone through by him with some minuteness, and others of his own device were employed. It is enough to look at the list of works, Greek, Latin, Italian, German, French, and English, to all of which he had referred, and which are enumerated at the end of the book, to judge of the literary labour which was involved in the historical portion of his work. He would spend whole days in the reading-room of the British Museum, and then sit up till three and four o'clock in the morning working again, and utilising the material which he had acquired, and this merely for the sake of giving completeness to the historical part of the subject upon which he was writing. Anstie was one who never relaxed his grip upon a piece of work until he had finished it, and so night and day, week after week, through the summer and autumn of 1863, he toiled in library and laboratory, at bedside and in out-patient room, gathering the materials for his book. What can I say here of this work? Its title is quite inadequate to give an idea of its contents, which embrace questions of the deepest interest in physiology and chemistry, psychology and metaphysics, besides problems of clinical medicine, and investigations in anæsthetics.

The book was successful, and amongst thoughtful readers made the reputation of its author, but it was never so widely appreciated as it deserved to be. Yet it had many lay readers too. I know that the author had the intention of condensing it materially, and presenting his argument in a more succinct form, and it would then probably have attained a much wider circulation.

Some of the experiments which he performed were attended with considerable personal risk. On one occasion he fastened to his face an inhaler containing an ounce of ether, placed his watch before him, took a pencil in hand, and began to make a mark upon a piece of paper on the lapse of every fifteen seconds. He soon lost consciousness, and when he could again distinguish his watch, he found that thirty-five minutes had elapsed, and that he had made but two marks on the paper. He was alone during this experiment. Another time he took a grain and a half of a very good extract of belladonna on going to bed, and woke in a state of delirium.

The mode in which various symptoms commonly referred to stimulation are shown to be really due to a narcotic or paralysing influence is one of the most interesting parts of this book. We instantly recognise the truth of his explanation of inebriation (as illustrated by the action of alcohol, chloroform, and hashish), that its essence consists in the destruction of the capacity of the brain for retaining or recalling moral and prudential impressions, and also for any kind of continuous intellectual labour. The apparent *excitement* of the emotions and desires is in truth but the unveiling the lower part of our nature, which is more or less ready, in each of us to spring into action when the customary checks are removed. Floating through the book too is the idea with which the author's mind was so strongly filled, that it pervaded all his later writings; the idea that food in that form in which it was capable of reception and application formed the most important part of therapeutics. I can remember, referring for a moment to my own experience, that this feature of the work came as a revelation to me, and it always seems to me that I learned more of useful therapeutics in general from his work on "Stimulants and Narcotics" than I have ever been able to derive from any other book. As regards the elimination of alcohol, the result of the experiments he made went to show, as did those performed by M. Edmond Baudot, that the colour test employed in their researches by MM. Lallemand, Perrin, and Duroy was so delicate that no fair inference as to the quantity of alcohol transmitted was possible, a very minute portion, even $\frac{1}{120}$th of a grain of absolute alcohol, producing a decided effect upon a cubic centimeter of the chromic acid solution which they used. M. Baudot had proved that, provided the dose of alcohol taken had not been excessive, no result capable of being distinguished by the alcoholo-meter appeared in the urine voided during twenty-four hours following the ingestion of the substance. Were the conclusion arrived at by Lallemand and Perrin correct, viz., that all the alcohol taken into the body was expelled unchanged within a short time, the alcoholo-meter would be capable of showing it.

Amongst the friends whose aid, in various directions, Anstie acknowledges in the preface to his book, was Mr. H. W. Chandler, Waynflete Professor of Moral and Metaphysical Philosophy in the University of Oxford, who had helped him, I think, in the references to Aristotle and in other points of metaphysical interest. Although they met but seldom, a warm friendship continued between these men, supported mainly by occasional correspondence, for the eleven years that remained of Anstie's life. I have received from Mr. Chandler a letter which at my request he kindly permits me to publish, and which I am sure will be interesting to my readers as conveying the impressions produced upon an accomplished scholar and university professor, who regarded Anstie from a position outside the medical profession. Mr. Chandler writes :—"I do not believe that there has been a day since Dr. Anstie died on which I have not thought of him and regretted his loss. Living as I have done a good deal alone, my experience of men has not been very extensive; but I am quite clear on one point—that physicians, medical men in general, are the most completely real *men* of all the different classes I have had to do with. I am not blind to what I think their weak points—far from it; but for all that they seem to me to stand in the foremost ranks of men, taking that word in its best sense. The reason seems to be partly that their profession forces them into most intimate contact with living and especially with suffering men and women, whom they see with their masks off; and I feel convinced that a man or a woman thus stripped as it were is both a much worse and yet without any contradiction a much better being than most of us suspect. Of all the men I have ever known, Anstie was, as it seemed to me, the most completely cut out by nature for his profession. Any other would probably have spoilt him and made him disagreeably lopsided. In medical theories or practice he had an object on which his pugnacious propensities (rather strong, if I am not mistaken) could expend themselves, while his experience of what pain was, his knowledge of the very mixed nature of men and their constitutions and motives, prevented him from damaging them if by any means it could be fairly and honestly avoided. I have seldom met anyone who had such a keen scent for humbug and pretence, or who more heartily hated it; yet it always seemed to me that underneath all his fierceness there was a deep under-current of kindness and even sympathy for the deluded and besotted creature who pretended to be what he was not, or to know what he did not know. His zeal for mere science—for mere knowledge as such—was always delightful to me; yet I think he would have been puzzled himself to say whether after all his bright eager desire to know was not really and at bottom a desire to help and save the sick and suffering and foolish. It always seems to me that you can form a very good estimate of a man's general character from the way in which he fights: I mean the way in which he conducts himself in controversy whether in words or on paper. Anstie was a gallant fighter, and cared not one pin for anything but the truth. If that won he was content, and with such a result he would, I feel sure, have regarded his own discomfiture as a thing to rejoice over. He and I were engaged in a desultory and most amicable war when death carried him off. How the verdict would have gone I don't know; but a more honest or fairer opponent no one could wish to have than he was; in fact, his idea of a controversy was that it was a mutual hunt for the truth. Had circumstances made him a lawyer, I think this transparent honesty of his would have run considerable risks. On the whole I am inclined to believe that his was one of those ardent, impetuous characters which might have hardened into something very unpleasant had it not been that his profession called out all the best feelings of a naturally warm heart, and so enlisted all the dangerous elements in him on the side of actual men and women with whom he so keenly sympathised. . . . . I can't tell you how I felt his death, for he was of all my friends the one to whom I should have gone by preference for help in any trouble of my own. One could have absolute confidence in him. I hope I am not unduly suspicious, but it seems to me that to say this of any one is to say all that can be said by way of commendation."

It was through his friend Dr. Russell Reynolds, who was at that time preparing to bring out a "System of Medicine," that Anstie was introduced to the Messrs. Macmillan; and "Stimulants and Narcotics" was the first medical work published by that firm. The book appeared early in 1864, when the author was engaged, amongst other work, in preparing a course of lectures on Materia Medica and Therapeutics, which he delivered at the Westminster Hospital School during the summer session.

In *Macmillan's Magazine* for February 1865 an article appeared on "State Medicine," by Francis E. Anstie, M.D. The subject was one which had long engaged his attention, and in this essay its principles are exhaustively considered. It is shown that for the State to decline to inquire about such matters as the bodily habits and health of its members is not "in favour of liberty," but in favour of immorality, dirt, disease, and death. The necessity of the appointment of specially qualified persons to report and advise in questions of lunacy, sanitary police, food adulterations and food inspection, as well as to give evidence on criminal trials and in coroners' inquests, is insisted upon by the author, who exposes, at the same time, the inefficiency of the existing arrangements by which the medical officer of health may be the nominee of some vestry which includes amongst its members persons responsible for the grossest derelictions of duty in regard of the sanitary state of houses belonging to them. Mr. Rumsey referred, in his paper on State Medicine, to this very able and well-reasoned article, and quoted from it two years afterwards in his speech upon the subject at the General Medical Council.

I think the publication of this article was very characteristic of Anstie. It was the genuine emanation of a strong feeling upon the subject, without the least *arrière pensée* as to its appearance advancing his own interests. In many respects, indeed, the suggestions which he made, in so far as they were opposed to time-honoured customs—notably to the rule of calling upon private practitioners to give skilled evidence in the coroner's court—were likely, as he knew well enough, to meet with the disfavour of many of his professional brethren—of those, at least, who were too shortsighted and selfish to see beyond what they might suppose to be their own immediate advantage. He felt, however, that the whole subject was in strong need of reform, and he threw himself with ardour into the task of advancing it, employing his vigorous pen in the cause, not only in this particular essay, but in many an unsigned article in a medical journal.

But the time was approaching when Anstie should be brought more prominently before the public than he had as yet been, and should lend important help towards reforms in the condition of workhouses, quite comparable with those achieved by John Howard in respect of the inhabitants of prisons nearly a century ago.

In October 1864 a country labourer, named Timothy Daly, who was suffering from rheumatic fever, was taken into the infirmary of the Holborn Union. A few weeks afterwards he was removed, at his own request, to a private lodging, and was subsequently received into St. Bartholomew's Hospital, where he died on the day following his admission. An inquest was held upon his body, which presented extensive bed-sores, and these, according to the evidence adduced, had occasioned his death. The coroner's jury found that whilst he had been in the infirmary of the union the deceased had not received sufficient care and attention. The typical blots of the workhouse infirmary system were exposed at this inquest—the ghastly deficiencies in all the varied essentials of hospital accommodation. Public opinion was much excited by the details (as it had been, for a few days, over and over again in previous cases), and the Poor Law Board was tardily driven to investigate a calamity the possibility of the occurrence of which it was its proper function to have prevented.

The matter in all probability would have gone the usual road of such incidents, and passed into oblivion, but for the circumstance of the recurrence shortly afterwards of a very similar scandal in another infirmary. The case of Richard Gibson followed that of Timothy Daly so closely, and the defects in nursing, medical supervision, and general care which were again laid bare were so terrible, that the subject rekindled the ashes of public indignation, and, what proved to be of more importance, roused the conductors of a journal which had often done good service before by its Commissions to undertake an investigation of the workhouse infirmary system.

In April 1865 the *Lancet*, commenting upon these two cases, announced that it had appointed Commissioners, whose task it would be to compare the system of the workhouse infirmaries with that of the public hospitals. The Commissioners were Mr. Ernest Hart, at that time sub-editor of the journal, Dr. Anstie, and Dr. Carr. I can well remember talking to Anstie immediately after Mr. Hart had gone over with him the rough preliminary sketch representing the main objects of the inquiry. Anstie was excited with the project, and lost no time in setting to work, and elaborating a scheme which embraced a more minute and detailed investigation than had at first been intended. The labours of the Commission were exceedingly heavy, and the largest portion of the work fell to Anstie's share. It might have been anticipated perhaps that a good deal of opposition would have been shown to the admission of non-official inquirers; but this was hardly the case. Of thirty-nine unions which were set down to be visited, no less than thirty-six were readily opened to the Commissioners; in two there was some delay in complying with the request, and only one refused to permit the visit. The visits were sometimes made by a single Commissioner, at others by two of them, and but little over two months, I believe, was occupied in a task which necessarily, therefore, involved very close application.

On July 1st, 1865, there appeared a report of the results of the investigation, which occupied seventeen columns of the *Lancet,* and was written by Anstie. It is a remarkable production, both in regard of the extraordinary grasp of the subject evinced by the writer, and the singularly graphic style of its composition. Written, as I well remember, *currente calamo,* with but few erasures, and at little more than one or two sittings, it is a model of a thoroughly practical report upon a subject which the writer had evidently studied by the light of the most advanced scientific information. Whence came this information? Anstie had never previously, so far as I am aware, had much personal experience of a practical kind to guide him; and any preliminary acquaintance with sanitary requirements which he possessed he must have picked up by yielding to an insatiable thirst for knowledge which prompted him to extensive reading, and which was associated with an extraordinary power of sifting what he read and of assimilating that which was valuable. In this inquiry he seems to have surveyed the ground with the eye of a general, perceiving exactly the points of importance, to have rapidly gleaned from authorities enough information to start with, and, then, in the progress of his work, to have gone on continually acquiring fresh knowledge. Commencing as a student, he finished as a master. The paper concluded with suggestions in reference to the alterations which were imperatively required in the infirmary system; and it was followed during the half-year by separate reports upon each of the various infirmaries which had been the subjects of investigation by the Commissioners.

Before the year was over beneficial results had already followed the publication of the reports. At first, as might have been expected, the statements were met with much indignant denial from the respective boards of guardians, and the writers encountered a good deal of abuse. But the public press in general had taken up the subject, and the labours of the Commission had met with the most gratifying reception at the hands of journals of all shades of opinion.

The work of the *Lancet* Commissioners and their followers, to whose united action the great changes in the workhouse infirmary system are owing, only began with the publication of these reports; and Anstie found himself involved immediately in continual labour connected with the further progress of the reform which had been so energetically inaugurated There were frequent interviews with public authorities, lengthy conferences with private persons who were eager to aid in the cause, attacks which had to be met, correspondence without end. But all this was insufficient to satisfy his energy. In the *Lancet* of June 10, 1865, he published a long letter on the "Distribution of Fever Cases in General Hospitals," strongly deprecating the custom of mixing typhus cases with the general patients of hospital wards. Later on, in September, there appeared in the same journal a paper on "The Physiological and Therapeutical Action of Alcohol," in which he referred to observations then being made with the sphygmograph of Marey by his friend Dr. Burdon Sanderson and himself, which encouraged the hope that a new and very delicate indication of the propriety of commencing or continuing the use of alcohol might hereafter be available. He insisted in this paper upon the importance of the proportional dose, and cited Professor Béhier, of Paris, as having been led to perceive that the *a priori* arguments against the use of alcohol were worthless precisely because they overlooked the differences introduced by variations in proportional dosage. He denied that the profession had decided, or was likely to decide, that the early and bold use of alcohol in acute disease was mischievous. In October the *Journal of Mental Science* contained a paper from his pen on "Insane Patients in London Workhouses." This represented the results of an independent inquiry into the arrangements for housing and treating pauper lunatics, in the course of which he had inspected the insane department of every London workhouse.

Meantime, at every available moment since the beginning of the year, Anstie had been working with the sphygmograph, at first in conjunction with Dr. Burdon Sanderson, who had preceded him with experiments in November 1864, and then independently; the object being to observe the effects of various substances, including alcohol, upon the sphygmographic tracing of the pulse. In November of this year he read a paper upon the subject at the Medical Society of London, and this was the first occasion on which the instrument was publicly introduced to the medical profession in England. Later in the same session he delivered the Lettsomian Lectures at this society, taking as his subject "Certain painful affections of the fifth nerve." In these lectures, afterwards published in the *Lancet* (June 16, 1866), the author introduced many of the views upon neuralgia which were afterwards expanded in his later writings upon the subject. Early in the summer he had been elected a Fellow of the Royal College of Physicians.

Thus closed the year 1865, which I have described in greater detail than I propose to repeat, not because the work was exceptional in amount, but, on the contrary, because the account may be taken as a fair example of the varied and arduous occupation in which Anstie delighted. It must be remembered that throughout this period he was besides a constant attendant at his hospital, working with ardour for many hours twice a week in an underground room, where he saw a large number of out-patients; that he had his lectures to give at the hospital school; and that scarcely a week passed without his contributing to the *Lancet*. Such an amount of varied labour would have been impossible but for his possessing, which he did to a remarkable extent, the power of turning rapidly to a fresh subject, and concentrating his attention upon it. So intense was this mental application, that the small matters of daily life were frequently either performed by him automatically or forgotten altogether, and an idea of dreamy abstraction was thus sometimes occasioned which was without foundation in fact. This "absence" was simply owing to the circumstance that his mind was actively elaborating something which never failed, sooner or later, to take form. His was not a nature to indulge in those misty speculations which the indolent mistake for useful creations, and which end in the dreams that give birth to them.

Rumours of the advance of cholera from the East had been prevalent during the autumn of 1865, and that vague but well-founded expectation of a coming outbreak of the disease in England which had preceded its arrival in former epidemics was again entertained during the winter. In the January number of the *British Quarterly Review* there appeared an *à propos* article on the subject of epidemic diseases, which Anstie had somehow found time to write in the midst of all the other work which I have described. This contribution was expanded and published a few months afterwards, as a book bearing the title of "Notes on Epidemics; for the Use of the Public." It is written in simple language, its main object having been to furnish information which might assist the non-medical public to do their part in the work of prevention. The book must be regarded as unusually successful in presenting scientific truths in a clear and intelligible manner.

Anstie's interest in questions of public health was always very keen, and his grasp of the whole subject was vigorous. It was about this period of his career that circumstances, especially the *Lancet* inquiry, threw him more than usual into this path of study, but the taste for work of this kind was innate in him, and was doubtless to some extent an expression of his vivid humanity. The field for helping his fellow man, which ordinary medical practice presented, was insufficient for the satisfaction of his instinct for assisting those who were weak or afflicted. This tenderness for suffering in any form crops out again, it seems to me, in his choice of "neuralgia" as a subject of investigation, and of therapeutics as the branch of medicine to which he most applied his mind. It was the relief of pain which perhaps of all subjects interested him the most, though he delighted generally in the treatment of disease, being singularly free from that pride in mere daintiness of diagnosis which in some men entirely usurps the place that should at least be shared with practical therapeutics. He loved remedies—whether they consisted in public measures for the amelioration of social distress, in modes of improving the physical and moral health, and thus preventing the development of some disease to which there was strong inherited predisposition, or in drugs by which especially pain could be alleviated. This practical tendency was always a notable part of his character, and, there can be no doubt, constituted one of its peculiar charms. His intellectuality, remarkable as it was, did not assert itself stiffly and coldly, as is not unfrequently seen, but rather allowed itself to be discovered by those who had been attracted by his intensity. It seems always to have been so with him. A friend of his and of mine—the

Rev. Henry Wace—chaplain of Lincoln's Inn, writes me:—"I knew him first at King's College when we were students there, he being rather older than I; and I remember how, whenever he intervened in our Debating Society, it was always to throw some enthusiasm into our discussions, and to sustain any thought which might lift them out of the region of merely intellectual debate. It was not the quality which is commonly called earnestness—a kind of over-conscious morality—but an instinctive ardour of the whole man in the pursuit of lofty aims, and perfectly compatible with the freest play of the genialities and the humour of life."

In the investigation of country workhouses, which the *Lancet* carried out as a sequel to its examination of the metropolitan institutions, Anstie did not take an active part except in one signal instance, that of Farnham, the examination of which establishment he was especially requested to undertake, as there was reason to think that its condition was unusually bad. What he saw and learnt at his visit (in which he was accompanied by Dr. Stallard) he described most graphically in a report which was published in the *Lancet* of October 19th, 1867. The account was singularly bold and outspoken, and the state of neglect which was exposed exceeded that which had been found in any metropolitan workhouse. The report caused great excitement, and in November a public inquiry was commenced with all the forms of a court of justice, a Poor Law Commissioner presiding, and the *Lancet* on the one side and the Board of Guardians on the other being represented by counsel. The result quite justified Anstie's report, and the direct consequence of the revelations at Farnham was to rouse an entirely unwonted activity at the Poor Law Board, and to cause the most stringent instructions to be issued to scores of country workhouses, involving changes larger than any which had been made since the New Poor Law came into existence. As evidence of the admirable and effective manner in which the *Lancet* Commission did its work, I need only refer to the vast changes which have been made in the whole system of metropolitan workhouses since light was thrown upon their state by the investigations conducted by observant men, in which Anstie took so prominent a part.

Nor has less success attended one of the last philanthropic efforts in which he displayed his accustomed energy. It was in the beginning of 1874 that Anstie brought before the College of Physicians a motion which he allowed was of a very unusual character. It was, that the College should petition the Premier to provide some remedy for the great and increasing overcrowding of the population in the metropolis. He explained that, during the last few years, the railway companies especially and the Corporation of London, in the interests of the general community, no doubt, had destroyed multitudes of poor habitations without making any considerable effort to supply their place, and suggested that no true remedy could be provided till some authority should be established which should have the power to supervise all improvement schemes, and to insist upon the carrying out of compensatory building before allowing existing habitations of the poor to be destroyed. The Charity Organisation Society was preparing to move in the same direction. Anstie was able to state that he had the sanction of the President (Sir George Burrows, Bart.) in bringing the matter before the College, and this was fortunate, for there was sufficient opposition on the part of some of the older Fellows of the College (who appeared to be alarmed at the novelty of the course which was asked to be followed) to have threatened the failure of his motion had it not been supported so influentially. As it was, the motion was carried, a petition was presented to the Premier by the Executive of the College in due form, and the result was that immediate attention was directed to the subject by the Government. In the following session there was passed "An Act for facilitating the Improvement of the Dwellings of the Working Classes in large Towns." Mr. Cross, the Home Secretary, in moving for leave to introduce the bill, quoted from the petition of the College, and made the following remarks:—"So deeply did this eminent body feel that the condition of the dwellings of the poor in the metropolis deserved the special attention of the Government, that they stepped out of their way last year to present this memorial to the Prime Minister which I hold in my hand." Under the Bill, which has now become law, corporations may, on the report of their medical officers, acquire buildings by compulsory purchase for the purpose of improvement. They may either build or let the land for building in accordance with schemes which they are to prepare with special regard to the accommodation of the working classes. Since the passing of the Act the Metropolitan Board of Works has received applications for an immediate exercise of the new powers.

The action which was taken with such vigour by Anstie is probably destined to have a far more extensive and beneficial influence even than the revelations of workhouse mismanagement which he published, for its indirect effect will be to prevent the pauperism of which his previous investigations had displayed the horrors. To have occupied such a prominent place in the carrying out of two great reforms within a period of ten years, is surely a very uncommon fortune to happen to any man. It is interesting to note, although this I suppose is not such an unusual circumstance, that this great public work was done by one who held no official position. When a chair of Public Health was instituted at University College in 1869, Anstie, to whom, as I have already described, was owing in no inconsiderable degree the propagation of those clear ideas upon the subject which had rendered such an appointment necessary, became a candidate for the post. To the great surprise of some of his friends and his own keen mortification, he was not appointed. There were those, however, who appeared to have formed a high estimate of his abilities in the science of public health; for it was not long before he was sounded as to his willingness to employ his remarkable qualifications as a sanitary officer in the public service. But an inspectorship at Whitehall would have involved his forsaking the pursuit of clinical medicine, and this he was not able to think of for an instant. I may illustrate the esteem in which his knowledge and judgment on sanitary matters were held by those qualified to form an opinion, if I note the constant appeals for his counsel and criticism that were made to him by his friend Buchanan, who ranked him, in respect of sanitary affairs, with the foremost thinkers of the present, and found in him qualities that must go to make the sanitary statesman of the future.

In July 1868 Anstie, in conjunction with Dr. Henry Lawson, had undertaken the editorship of the *Practitioner*, a journal which was established for the purpose of affording a medium for the scientific prosecution of therapeutics. After the first year he became sole editor of this journal, and retained the post up to the time of his death. How successfully he conducted his office the readers of the *Practitioner* well know; but the amount of work which it entailed may not be so apparent. Not only did he himself contribute very numerous papers to it, but, unlike what is customary, the reviews in the *Practitioner* were almost without exception from the editor's own pen. I do not think that one in a hundred of these were contributed by others. Anstie's reviews, it seems to me, were much more characteristic of the writer than is usual in a branch of literature which, at least in medical journals, is too often chiefly remarkable for the dead level of indiscriminate praise which is bestowed upon the authors of books. There is an unusual amount of life and energy in his criticisms. The writer is full of enthusiastic love of his art, and, evidently thinking it no light offence when an author tries to foist his stale platitudes or inaccurate statements upon unsuspicious readers, either denounces the attempt with indignation or, perhaps more frequently, lashes the offender with unsparing ridicule. These occasions often give indications of the sparkling humour of the writer, a quality in Anstie which was often quite unsuspected by those who only saw him in the fire and heat of some burning question.

Quick as he was, however, in perceiving and exposing that which was dishonest in the works that came under his criticism, he was yet a most sympathetic judge, for his sanguine temperament carried him half-way towards the reception of novelty in treatment. Many of us, whom bitter experience of previous disappointments has inclined to be sceptical about most things which are new, are often unable to see reason for expecting any important difference of action between this or that form of a particular remedy. Anstie, on the contrary, under such circumstances would by no means begin with disbelief, but would welcome the hint and try the experiment for himself. Of course a nature such as his always runs the great risk which belongs to jumping at conclusions in the hurry with which its judgments are formed, and he was not altogether free from this fault. But as we turn to his papers and books we must be surprised to find how little he has sinned in this respect, and must own, I think, with gratitude, the increase of faith in medicaments which we owe in great part to the contagion of his hopeful spirit. It is always a delicate matter when the particular share which one person has taken in a movement is the subject of discussion, and it would be indeed painful to me were I to commit the fault of exaggerating Anstie's claims upon us in any respect. But when we call to mind the almost exclusive attention to morbid anatomy which marked the tone of the profession during several years following the foundation of the Pathological Society, the mode in which the question of therapeutics was often waived as really one of nursing and expectant attention, and then remember the constant war that Anstie waged against this state of things, which he denounced as an "irrational scepticism," and the variety of suggestions in therapeutics which he never tired in bringing before us, we must own, I think, that the present improved position and hopeful prospects of the healing art proper, in England at least, are in no small degree owing to his energy. Even in the matter of appliances which did not originate with himself, such, for example, as the hypodermic injection of remedies and the scientific use of electricity, his active and inquiring spirit was always extending the field of their utility, whilst the diffusion of the experience he was gaining was largely helped by the singularly graphic and delightful style in which he wrote.

The application of remedies, however, was but one portion of the therapeutic work which he contributed. Some of his papers, notably those "On the influence of inheritance upon the curability of nervous disease,"[1] "On the use and abuse of alcohol by women,"[2] "Indiscriminate stimulation in chronic disease,"[3] "On the hereditary connection between certain nervous diseases,"[4] embrace likewise questions of profound interest in reference to the prevention of disease, and various psychological problems of the highest importance in relation to social science. He felt and expressed himself strongly on the subject of the education of youth, and was in the habit of protesting with indignation against a far too prevalent custom of stimulation of the emotional part of the mind. There was a peculiar delicacy and humanity in the mode in which he dealt with all subjects which require to be approached from a moral as well as from a physical point of view. This is evident in his work on "Neuralgia" as well as in many of his papers.

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[1] *Practitioner*, Vol. IX. p. 193.

[2] *Ibid.* Vol. VI. p. 87.

[3] *Ibid.* Vol. III. p. 34.

[4] *Journal of Mental Science*, January, 1872. The substance of this paper was delivered as an address at a Conversazione of medical men of the West Riding, at the West Riding Asylum, Wakefield.

The subject of Public Hygiene was ever in his mind as one to the importance of which journalism had heretofore failed to give sufficient prominence, and it was with no small feeling of satisfaction therefore that he found himself early in the year 1873 in a position to establish a "Department of Public Health" in connection with the *Practitioner.* At the time that he announced his intended arrangement there was no journal in England which afforded the facilities for the intercommunication of sanitary workers which he was instituting. Since then the example has been followed, and now, I believe, several journals are published which are either wholly or in great part devoted to the subject.

Anstie contributed three articles to the second volume of Reynolds's System of Medicine: on "Alcoholism," "Neuralgia," and, in conjunction with Sir William Gull, on "Hypochondriasis." The articles in the third volume on "Pleurisy," "Pleurodynia," "Hydrothorax," "Pneumothorax," and "Hepatalgia" were also by his hand. He wrote the Report on Practical Medicine in the Biennial Retrospect of the New Sydenham Society for the years 1865–66, 67–68, and a series of lectures on diseases of the nervous system which appeared in the *Lancet* during the winter of 1872-73. These last, he told me, were instalments of a work on functional diseases of the nervous system which he had the intention of bringing out, and which he hoped to be able to follow up by another on the organic diseases of the same system. This list, however, necessarily gives a very imperfect idea of the large amount of literary work which Anstie got through during the last five or six years of his life, and which I am unable to particularise. His book on "Neuralgia and the Diseases that Resemble it" was published in 1871, and he was meditating a new edition of it at the time of his death. A peculiar value is lent to his writings upon pain from the circumstance that he himself had suffered severe and frequent attacks of neuralgia of the first division of the fifth nerve from an early period of his life. My space as well as other reasons cause me to abstain from criticism of his works, but I cannot help directing attention to the different position which the subject of neuralgia now holds in English medical literature to that which it occupied before Anstie turned upon it the light derived from his minute and accurate study.

With objects of interest tempting him on all sides, there was one subject which he never deserted. The mode in which alcohol is disposed of in the economy never ceased to engage Anstie's attention, although it was only from time to time that he was able to resume experiments in it. In 1867 he obtained leave to deliver at the Royal College of Physicians two lectures upon the prognosis and treatment of certain acute diseases, with special reference to the indications afforded by the graphic study of the pulse. The lectures were illustrated by numerous sphygmographic tracings, and in the course of them he referred to the alcohol question, detailing results which he had arrived at with the assistance of his colleague, Dr. Dupré (vide *Lancet*, August 17, 1867). He had succeeded in proving indisputably that when alcohol is taken in non-intoxicating doses, the total elimination in the twenty-four hours only amounts to a fraction (generally a small one) of a grain for the kidneys, and even smaller quantities for the lungs and skin. By a curious coincidence, as he learnt afterwards, it happened that a German observer, Dr. Schulinus, was investigating the same question simultaneously with himself, and his results closely corresponded with those obtained by Anstie and Dupré.

The help which Dr. Dupré rendered in these and other investigations was, I need not say, of the most valuable kind, and was thoroughly appreciated by Anstie, who ever spoke of it in the warmest terms, and neglected no opportunity of acknowledging his obligations to his colleague. When Anstie took up in the *Practitioner* the subject of the adulteration of drugs and other articles, it was Dupré who performed the chemical analyses that were published. For this work the journal was several times violently attacked, but Anstie, with well-founded reliance upon the accuracy of the report, would take up the fight and carry it out in a masterly manner, although the subject was one to some extent outside his proper work. It was through these articles in the *Practitioner* that her Majesty's Privy Council was induced to institute an inquiry into the condition of the retail drug trade in London, and I believe also to include drugs in the Adulteration of Food Act. During latter years Anstie found his way almost daily to the chemical laboratory of the Westminster Hospital, and when he entered with his genial smile, full of energy and bustle, inquiring how work was getting on, Dr. Dupré would at once feel, he tells me, the better for his visit, and somehow gained fresh energy from Anstie's apparent exuberance of it. He did not actually work manually in the laboratory, but would stand by, looking on, or pace rapidly about, asking questions, or consulting with his friend as to what was next to be done. He had the most thorough acquaintance with the literature of the work they were engaged upon, the question of the elimination or non-elimination of alcohol, as well as of its physiological bearings, and could therefore point out exactly what had been done and what remained to be determined. As is well known, besides the joint investigations in which they were occupied, they also worked upon the same subject independently, and each published papers upon it without the cooperation of the other.

In a scientific subject not familiar to him, Anstie did not at first sight perhaps appear to possess unusual power of apprehension, and Dupré sometimes thought that he had not succeeded in making the matter—some question in analytical chemistry—quite clear to him. He would stand and listen, repeating, "Yes, yes!" in an impatient tone, but make no other sign of comprehension. On subsequently reading the article which Anstie would write upon the matter, Dupré would be astonished to find how perfectly he had grasped the entire subject, and at the masterly way in which it was put forward. There is no doubt indeed that Anstie would think his subject out in the most thorough but rapid manner before he put a line on paper, and then he never seemed to need a pause. He was much interested in watching the extensive series of investigations in the chemistry of wine which Dupré made during the last few years, and conversed much upon the subject with his colleague. His articles in the *Practitioner* on the dietetic and medicinal uses of wine in health and disease arose in great measure out of these valuable opportunities of study, but they were entirely his own work.

Anstie was always foremost in his readiness to help in advancing the efficiency of the hospital to which he was attached. He was in the best sense of the word a reformer, not destroying the old simply for the sake of destruction, but ever ready to advocate the new, at any personal cost, if he thought it better than the old. Such views are not universal, and he consequently often met with opposition. But I believe that even those who came in for some of his hardest hits recognised instinctively that, however wrong he might be in his judgment, according to their notion, the integrity of his motives could not be challenged. With Anstie, an outburst either by spoken words or lines traced by his pen with immense rapidity, seemed to act like a lightning discharge in clearing the air. I have often seen him, furious at something which outraged his sense of what was honest and just, sit down and write a vehement letter of remonstrance, which he would read out with evident satisfaction, and then as often as not tear to pieces with a hearty laugh. Under such circumstances one remarked a special feature of his character in which he differed from a great many of us. After an explosion of this kind he would turn to some scientific work which he had on hand, take up, without the least loss of time, the thread of his subject, and in a few moments be as thoroughly immersed in it as if he had thought of nothing else for hours past.

The quickness of temperament thus indicated pervaded his character. Called to a patient, he must obey the summons at once, no matter how much his previous arrangements would be disturbed. An idea conceived must be acted upon without delay, whether it was a paper to be written, a room to be furnished, an experiment to be carried out, a present to be purchased. And in the midst of all his restless movement and the multifarious claims upon his time and attention, one would hear ever and anon his clear sweet voice carol out a few bars of an air; or he would suddenly call to mind some ludicrous incident which had excited his keen sense of humour, and be impatient to impart it to a sympathetic ear. So free was he from guile of any kind, that he did not even possess the art of concealing, for the time being, the working of his thoughts as he weighed the import of the symptoms in a case which he was investigating. His intenseness, under such circumstances, would lend to his face an air of such deep gravity as sometimes, until this peculiarity was known, gave needless alarm to the patient or his friends. Happily, however, the sanguine tone of his character counterbalanced, to a large extent, the effects of a trait which might otherwise have seriously interfered with his success in practice.

In conversation, when his interest was strongly excited, his impatience would scarcely allow him time to listen, but he would rush at what he thought was coming, and reply to a statement which was only half completed. Of course in this he was sometimes at fault, but he was far more frequently right, for he seemed to have that kind of facility in building up the tenor of a communication from the first few words of it, which we often, (to employ a strange analogy) observe in a deaf mute whose quick wit only needs to see a few movements of his companion's fingers to read his meaning through and through.

Anstie's untiring energy and interest in life took him into most things, and introduced him to a large number of the leading minds of the day, not only in his own profession, but outside its pale. Amongst these, and occupying an especially prominent place in his affectionate regard, was the Rev. Harry Jones, now rector of St. George's-in-the-East, at whose church, which at that time was St. Luke's, Berwick Street, Soho, he was a regular attendant. There were few Sunday evenings also during many years upon which he did not make one of a small circle which was wont to assemble at Mr. Jones's house for friendly chat, and there was no one who contributed more largely to the interest of a social gathering the simple pleasures of which the writer can never forget. How thoroughly his esteem was reciprocated is shown in some lines of a letter which Mr. Jones wrote to me on receipt of the news of my brother-in-law's death. "I loved him," he writes, "dearly; one of the truest and best friends and men I ever knew. . . . . Brave as a hero, and true as he was brave. Down in the front rank, with his sword in his hand, and his harness on his back, fighting on the side of the

God of health and life. We can ill spare such Christian soldiers. I can't tell you how I shall miss him. He always stirred my heart and helped me in trying to do right. Since the death of Maurice there is no sorrow that has come to me like this. But this is worse. Maurice had done his stroke of work."

There were probably few medical men of his time who had so extensive an acquaintance amongst the representatives of medicine abroad. Scarcely a week passed without correspondence with some learned professor, or a visit from some foreign doctor anxious to make the acquaintance of one already well known to him by his writings, and there was scarcely a limit to the genial hospitality with which he received such a guest.

For the purposes of his journal, as well as for his own instruction, he made himself acquainted with every important work which was published on the Continent or in the United States, and one would often see him, when too tired to write, plunge into a big German book, and acquire with great rapidity a very fair knowledge of its contents. He was never idle. Thanks to his constant following up of the progress of medicine in various languages, and his excellent memory for what he read, Anstie had, it always seemed to me, quite an exceptional knowledge of what had been done in medical literature, and the various directions in which investigators were moving. There was something eminently cosmopolitan, too, in Anstie's nature. A thorough Englishman in his outspoken boldness and love of fair play, he was singularly free from national prejudices, and foreigners of every country seemed to be at home with him at once. Especially was this the case with the Americans. Perhaps there was something in the independence which characterised him and his dislike to conventionality, that helped to this result: but it is a fact that he was extremely popular amongst his professional brethren both in the United States and in Canada, although he had never crossed the Atlantic. They only knew him through his essays and books, but they appear to have estimated his qualities very highly indeed. In illustration of this I may mention now, though the matter was to a great extent kept a secret at the time, that in the last year of his life Anstie received pressing and repeated invitations to occupy the chair of Medicine in one of the best-known and most important medical colleges in the United States. The temptation was a powerful one, for Anstie had a strong liking for his Transatantic brethren, but family circumstances especially served to influence him in rejecting an offer which to his mind conveyed the greatest honour that he had ever received. And here let me add another incident—touching in its *naïveté*—which was warmly felt by him to whom the compliment was paid. A doctor of the United States, powerfully attracted by Anstie's writings, by which alone he knew him, and having, as he wrote, "no child of his own," persuaded a patient to have her infant baptised in the names, "Frank Anstie!"

These fragmentary recollections of my dear friend would be more than ever incomplete without some reference, however slight, to the domestic side of his life, of which I saw much from the year 1863, when he moved to Wimpole Street, and necessarily more when, a few years later, I married his wife's sister. Those who only knew Anstie as the energetic worker and fiery champion of the right can form no conception of the almost feminine tenderness which marked the side of his character presented to those who were dear to him. To love and cherish those immediately belonging to us is not an uncommon virtue, and even the egotist often displays it to great perfection, but Anstie made the field for the exercise of this quality an extremely wide one, which included indeed all those of his connections who stood in need of any kind of help: and I know that on several occasions his house was freely offered as a home for such as were sick and required special care and attention. Absolutely unselfish, his own experience of the battle of life appeared to widen, if that were possible, his sympathy with the troubles of others, and increase his anxiety to aid them at any personal cost. As may well be imagined, his love of children and their attraction towards him were very strong, and he found in the Belgrave Hospital, to which, on its establishment, he was appointed physician, opportunities which were very dear to his compassionate nature for using his skill and kindness in their behalf. Nor was his tenderness less towards the lower animals; and I have now before me a little canary of which he was very fond, and which used to chirp to him as he worked. It was entirely characteristic of Anstie, and, at the same time, not out of real harmony with his love for men and animals, that, when the crusade against vivisection was lately commenced, he threw himself at once into the contest on the side of his profession, utterly regardless of any personal obloquy with which his championship of the cause might threaten him. An article which he published in this journal (May 1874), criticising severely the tone adopted by the *Spectator*, contained an admirable exposition of the subject. His adversary was a generous one, and wrote of him, after his death, "Not many months ago he attacked us very sharply in his monthly periodical, the *Practitioner*, for our denunciation of the atrocities of painful vivisection, and we inserted from him a vehement letter in rejoinder to our reply. But whether he was in the right or in the wrong, it was impossible to mistake the ardour of the man, or to doubt the complete and very unusual disinterestedness with which he threw himself into all his work. He was indeed a good member of the Church Militant, whatever his creed, for he loved to fight and even suffer in a good cause. Yet there was nothing narrow about him; his zeal did not arise, as zeal so often does, from exceptional limitations of temperament or knowledge. On the contrary, in spite of his wide attainments, the 'fire was not quenched.' Not only medical science, but both medical and human morality, have lost a strenuous champion in Dr. Anstie."

The relative pecuniary success of men in consulting practice is a subject upon which, in the nature of things, but little is accurately known, and I cannot say, therefore, whether there was anything exceptional in this respect in Anstie's career. But it seemed to me that, until the last year or so of his life, the amount of private practice that he obtained was small; and I know that he was very often discouraged at his progress, and extremely anxious for the future. I am not alone in thinking that the kind of work in which he had appeared before the public so conspicuously operated detrimentally to his immediate success in this respect. Still his progress had been steady, although it was slow, until the summer of 1874, when consultations with medical men came in so much larger numbers week by week that a turning point in his career had evidently been reached, and a path entered which must have led, had he lived, to his occupying a very advantageous position

He was engaged upon an inquiry of a very extensive kind in reference to the physiological effects of alcohol, which he had commenced in the winter, when this turn in his fortunes came upon him. He had planned a number of experiments, in which it was meant to determine the amount of any alteration which might be produced in any of the vital or, perhaps more correctly, vegetative functions. Such experiments were to include the effect of varying doses of alcohol on the temperature in health, and upon the pulse. His previous observations with the sphygmograph were to be repeated with the improved instrument now in use. In this the adjustment for the pressure of the spring being known for each case, the results can be made comparable. Fully appreciating the want of some more sensitive indicator of temperature changes than the ordinary clinical thermometer, he had intended testing the utility of the thermo-electric pile in these experiments, although he expressed some doubt whether observations on the surface of the body, even with so delicate an instrument, would give a correct view of any changes of temperature due to the alcohol. Besides these physical determinations, a very complete series of chemical experiments was planned, the object of which was to ascertain any change which might be produced in the excreta of the body, either as to the amount of their normal constituents or the possible production of fresh substances. He had already published some results of the preliminary part of this inquiry which were of extreme interest, as showing the possibility of a total absence of urea-increase during the course of an intense pyrexia of considerable duration. The observations were being conducted at several of the metropolitan hospitals, by the courtesy of various members of the medical staffs; and he was assisted by Mr. Frederick J. Hicks (formerly assistant at Oxford to Sir Benjamin Brodie) in the chemical analyses which they involved. His aim was to investigate what effect alcohol might produce upon patients excreting daily an abnormal amount of urea, as in cases of typhoid fever, pneumonia, erysipelas, gout, and rheumatism, and other diseases attended by elevation of the bodily heat.

The following experiment was also one which he had intended making:—In a healthy dog, fed with a constant amount of food (the quantity of nitrogen contained in this being accurately known), to determine (1) the total nitrogen in the urine, and (2) the quantities excreted during several successive hours after the administration of varying doses of alcohol.

It was also suggested to determine the quantity of carbon in the food, and to measure the volume of air supplied to the dog. Then, knowing the weight of the animal before the experiment, it only remained to weigh it again afterwards to have all the facts as to the ingoings and outgoings, viz.—

Weight of nitrogen, carbon, oxygen of air, and other constituents of food supplied.

Alteration in the weight of the dog.

Weight of nitrogen and carbon excreted.

Weight (total) of solid and liquid excreta.

In this way it was hoped that, even if the actual nature of any changes produced by the alcohol were not discovered, any marked alteration must certainly be detected, and a clue might thus be obtained to the proper direction in which other experiments should be made.

With his experiments and observations, his own hospital work (he had not long been promoted to be physician) and lectures, with his increasing practice coming, as I have indicated, with that kind of rush which means a great absorption of time, with the *Practitioner* to be provided month by month, and other literary work of various kinds which he was never without, the long days of the summer of 1874 were yet all too short for what Anstie was doing. And with it all there came an urgent request to him of a kind which he could not resist, for it appealed at once to his courage, sense of justice, and chivalry. An effort was being made to found a School of Medicine for women, and he was asked to be the Dean. He consented, and soon found himself involved in the laborious and often vexatious work of construction in the teeth of prejudice and opposition. He had, as I well know, no strong views upon the question, but he thought it would be ungenerous on the part of the medical profession to refuse such help as was requisite for the trial of that which he considered a pure experiment.

This laborious summer came upon Anstie jaded with the almost incessant work of years, and sadly needing rest. He had often told me during the twelve months or so that preceded his death that he found he could not write as he had formerly done —that he felt a want of the elasticity which had heretofore possessed him. There was little outward change, however, to be noticed in him. Bright and genial, always working at high-pressure, and yet always ready to do anything for any one, his energy and restlessness made him appear almost ubiquitous. In spite of all his occupation he never shirked social claims, and there was rarely a meeting of the Clinical Society at which he did not attend and take part in the discussions. But although in later years his figure had increased in bulk, and he looked in some respects the picture of health and strength, there were many of his intimate friends who felt that the pace was too great, and often urged him to husband his powers for the work which the future would certainly have in store for him. In the course of the summer he took a house at Finchley, to which he sent his family, hoping, from the easy distance, to be able to join them frequently. But it was only on rare occasions that he succeeded in obtaining even so slight a holiday as this.

He dined with me on the 4th of September, after his first visit to the school at Wandsworth, where he was destined to contract his mortal illness whilst investigating the cause of a fatal epidemic which had broken out among the children, and was full of excitement and interest in the inquiry which he was commencing. I saw him next morning when he ran in for a moment as I was starting for the country, and I saw him again, a week later, when the telegraph summoned me to his death-bed. He was delirious, but gave me one look of recognition; and in an hour he whose presence had always carried with it the idea of life in its most intense form was no more. His work would never have ceased so long as he had retained the power to perform it—but it was over. Shall we grudge him his rest?

THE TREATMENT OF QUINSY.

It has for several reasons seemed desirable to know what is the actual practice of the profession in their treatment of ailments; and in order to ascertain this the Editor of the *Practitioner* issued, some little time ago, the first of a series of circulars which it is his intention to send out from time to time. This first circular inquired as to the management of a well marked malady, about the diagnosis and prognosis of which there is little or no doubt. To this circular 112 replies have been sent in, one of them coming from Norway, and although this is by no means a large proportion, the Editor feels encouraged by the result, and takes this opportunity of returning his most sincere thanks to those gentlemen who so kindly complied with his request. The reception they have given to his scheme is so gratifying that ere long a second circular will be issued. The following analysis has been made of the different returns, and though such a statistical account is necessarily somewhat dry reading, we trust that it will not be found entirely without attraction for our readers. In some parts the information is not so definite as our readers may desire, and in the future we will endeavour to make the return more exact. In order to do so it will be necessary to modify the form of the questions in such a way that the answer will entail somewhat more trouble on the responding gentlemen, and we beg those who have so far so willingly aided us, to forgive this as we do it in the interests of our readers.

The first of the questions put is this: "What are the drugs you generally prescribe in this disease?" As might be anticipated the treatment is far from uniform. The widespread belief in the efficacy of guaiacum in tonsillitis is evidenced by its having the largest proportion of supporters, namely, 36. The doses and the forms of the drugs used will be given under the next division—the second question put. Next followed the chlorate of potash with 30 supporters, 11 of whom prescribe it in combination with the nitrate of potash, and 8 with percloride of iron. Aconite is third on the list with 27 supporters; 2 of whom give it along with belladonna. These three are the chief remedies in use, being given in 83 per cent. of the whole returns, or 93 out of the whole 112 who have replied to our questions.

Next, but at some distance, come saline purgatives, which find favour with 19; and then saline diaphoretics, advocated by 11 of the replying practitioners. After these come quinine, used by 6; while 4 more give it with iron. With 5 gentlemen the sulphate of magnesia with tartarized antimony forms the favourite measure; with others mercury with belladonna and the sulphide of calcium is prescribed. The least generally used measures are hydrochloric acid with bark (3); sulphate of magnesia with sulphate of iron (2); actea racemosa (2); Dover's powder (1); carbonate of baryta and opium (1); belladonna and chloride of barium (1); sesquicarbonate of ammonia (1); and mercury with turpentine (1).

The second question: "What preparations of these drugs and what doses do you generally prescribe?" elicited an equal diversity of practices, so far as the answers will permit of their being tabulated.

To take the most favourite measure, guaiacum, first: it would appear that the tincture in doses of from half a drachm to a drachm every 2 or 4 hours is in most use. In 9 answers it is given in half-drachm doses every 2 hours; by 2 every 4 hours; while 5 give a drachm every 4 hours. Others again prefer guaiacum powder, which is prescribed in half-scruple doses by 6, and in scruple doses by 2; 3 hover betwixt these doses; while 5 give 10-grain doses each of guaiac powder and chlorate of potash together, and 1 prefers 2 scruples of guaiac and 10 grains of the chlorate of potash. Little or nothing is said about the frequency of these doses.

As to the manner in which chlorate of potash is prescribed, it seems that 6 give 15 grains every second hour and 2 ten grains at this interval. Three give the dose of potash with 5 minims of dilute hydrochloric acid. With 2 the dose is 20 grains every third hour. The combination of the chlorate with the nitrate of potash in 10-grain doses of each, at intervals of 4 hours, found 3 supporters, and 10 grains of the chlorate with 5 of the nitrate also 3. Others prefer to give the chlorate with perchloride of iron, 4 giving 15 grains of the one with 15 minims of the other; while the other 4 prefer 15 grains with 20 minims.

In the administration of aconite the minim dose at hourly intervals found most advocates—namely, 15; while 4 more give one or 2 drops every hour. The old-fashioned plan is followed by others; 2 give 5 drops every 2 hours, and 1 ten drops every 4 hours.

Of the least common remedies of which any account of the dose is given, it appears that 9 give the perchloride of iron in doses of from 10 to 20 drops; 1 from 20 drops to half-a-drachm; and 1 gives half-a-drachm 3 times a day. Of those who prefer the sulphate of magnesia and tartar emetic, 4 give drachm doses of the sulphate with 15 minim doses of antimonial wine. Of sesquicarbonate of ammonia the dose is 5 grains; 1 giving it alone, while 2 add it to their other measures.

Quinine in grain doses finds 3 advocates, and the tincture of cinchona in half-drachm doses a solitary friend. Of the sulphide of calcium the dose is one-hundredth of a grain. The defective answers are conspicuous here, and yet it is an important part of the inquiry, as our readers will feel. Of those who advocate salines in different forms, 3 only say how they prescribe—namely, 15 grains of bicarbonate of potash every 6 hours. If our readers will but give us their friendly aid we hope to make this section more perfect in our future analyses.

The third question is: "What local treatment do you employ?" The variety of measures adopted by various practitioners is here again conspicuous. Poultices find most favour, and are advocated by no less than 40 of our answerers. Gargles make a close second with 39 supporters. Steam inhalations are largely used—namely, by no less than 29. The application of nitrate of silver, either in stick or in very strong solutions, is advocated by 17 practitioners; turpentine stupes by 12; ice by 9; leeches by 6; belladonna liniment by 5; the perchloride of iron 4; tannin and glycerine 3; cold wet compresses 3; and compound camphor liniment 1. It is impossible to give the individual practices of the different practitioners in their use of these varied measures.

The fourth question is: "What diet and regimen do you recommend?" and is answered by a large proportion. A simple diet of milk, slops, with a little beef tea, is advocated by 66 of our correspondents; while a simulating diet finds favour with no less than 30. In forming these two sets of respondents we must state, in order to be clear, that several of these last 30 are also ranged under the first class. We have given the exact numbers of the supporters of each plan according to their answers. As to separating the treatment into that suited to the pyrexia and that suitable to the convalescence, 6 allude to the second stage and advocate tonics in it; but there are no further distinctions made.

Such are the conclusions to be arrived at in the first section of our questions. The latter half comprises rather more detailed interrogatories, which however are not without interest.

The general question of this section stands as follows: "How is your choice of drugs and line of treatment modified by the following circumstances:—(*a*) The constitution of the patient; (*b*) age and sex; (*c*) symptoms; (1) pain in throat; (2) swelling in throat; (3) pains in limbs; and (4) constipation.

A considerable number of our correspondents stop their answers at this point, giving a generally negative answer, such as, they are not influenced by the matters mentioned. Others again give careful answers.

As to (*a*) "constitution of patient" (strumous, rheumatic, gouty, &c.) answers were furnished by 51 practitioners, whose replies were chiefly of a general character. Struma was alluded to in 14 answers, rheumatism in 9, and gout in 7; but little of a definite nature was said, or to which allusion can be made here. One gentleman remarks that the perchloride of iron with chlorate of potash "acts like a charm" in strumous cases; and a second says this combination is very good in such cases.

(*b*) "Age and sex" receives special notice by a few who confine themselves to the general statement that a less dose is required for women and children than for adult men. One refers to the need for stimulants in the case of aged persons; 1 of quinine in cases in advanced life; and 1 refers vaguely to the desirability of attending to uterine disorders.

(*c*) "Symptoms" evokes a much more general and also more definite response.

The first question under this head is (1) "Pain in throat." The measures resorted to for the relief of this symptom are as diverse and various as any yet mentioned. Poppy fomentations (9) and blisters (9) bracket for the first and second places; leeches (8) and morphia (8) are bracketed third and fourth; while poultices follow with 7,[1] and incisions 6. The less commonly used measures are—the cold wet compress with 3 advocates; cotton wool 2; chloral 2; bromide of potassium 2; while hot water is relied upon by 1; the æther spray by 1; iodine vapour and steam 1; while 1 is strongly in favour of hot hops.

(2) "Swelling in throat" elicits a fairly general response. The local application of heat and moisture seems in most favour, for hot fomentations take the first place with 14 supporters; steam inhalations follow close with 13; while belladonna liniment finds favour with 9. Ice is relied on by 4; while 1 advocates spongio-piline, and 1 advises no hypnotics.

It will seem that here, as in the answers to the general question of local treatment, heat and cold both have their advocates; cold however being markedly in the minority.

(3) "Pains in limbs." This question is answered definitely. but by a small proportion: the general answer being that this symptom is relieved by the treatment given above. Of the definite answers 17 advocate the use of Dover's powder, especially at bedtime; and 7 advise hot baths. Iodide of potassium finds 4 supporters: hyd. c. creta 3; aconite 2; bicarbonate of potash 1; and colchicum 1 advocate.

(4) "Constipation." This symptom meets with pretty general attention. As to the use of different measures to relieve this condition no less than 35 gentlemen resort to saline purgatives;

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[1] These are all that mention poultices here. It will be seen by looking back to question 8, that poultices have no less than 40 general advocates.

12 prefer enemata; 5 put faith in calomel and jalap; and other 4 in senna. Colocynth has 1 supporter; castor oil 1; croton oil 1; pulv. jalap co. 1; calomel and James's powder 1; while 1 uses scammony giving from 4 to 6 grains in milk. Several in answer to this question state that the guaiac is sufficiently purgative.

The last question is: "To what extent do you resort to surgical interference?"

The diversity of practice of different medical men is well seen in the answers given to this question.

Scarification to arrest the advancing tonsillitis has 21 supporters, who prefer it to any other form of local treatment. In some answers the practice is deprecated.

As to opening the abscess it finds 62 advocates, of which 4 say that they resort to it as soon as possible; while 13 state that they almost never use the knife. In no less than 17 answers it is stated that it is never required; while 4 state that they have never seen an abscess form since they have adopted the guaiac treatment. Such are the conclusions furnished as to the actual practice of the profession in their treatment of an affection which is so easily diagnosed, and about whose progress and prognosis their exists no difference of opinion. It will be seen that some hold to the old fashioned practice of sulphate of magnesia and tartar emetic, while others advocate minim doses of the tincture of aconite every hour. In this latter method we see the growing importance of maintaining a steady and regular action by frequent repeated doses, instead of the distinct blows at intervals which was the prevalent practice of of yore. The latter practice is supposed by some to savour of a *soupçon* of homœopathy, but such is not the case. The importance of substituting the small but continuous dose for large doses at longer intervals is as well recognised by orthodox members of our profession as by the homœopathic practitioners; and the silent pressure of opinion around us is in favour of such usage in the treatment of many maladies.

In estimating the number who use aconite it must be borne in mind that the number 27 does not comprise merely those who use aconite solely; but includes the whole number of those who mention it at all, as part of their measures. It will be seen that the number of advocates of every measure in the aggregate amount to 140, or 28 more than the whole number of returns; while 168—or 56 more than the whole returns—are given as the total number of answers to question 3. The numbers represent the answers given, and it would be impossible to arrange the answers in any other way, without returning them to the different respondents for more accurate details: a measure which is simply out of question for the valid reason that the large majority of the returns are unsigned.

Since the above has been in type 8 answers have been received from the United States of America. Very curiously guaiacum is never mentioned by them in any form. The most favourite remedy is quinine, which is recommended by 5 writers, one of whom mentions that sulphate of cinchonidia forms an efficient substitute. Aconite and belladonna, are advocated by 3 writers, one giving them alternately. Chlorate of potash with iron 1; veratrum viride 1; and iodide of potassium and quinine is strongly advocated by 1. In one case ipecacuanha is recommended along with aconite and morphia. The tincture or fluid extract of Phytolacca in 10 drop doses is considered by 1 to be almost a specific. As to the dose, 1 gives the aconite in 10 to 15 minim doses every three or four hours; another 20 minims every third hour. The iodide of potassium is given in doses of from 20 to 60, and even 100 grains three times a day, with 2 grains of quinine every third hour. As to local treatment they are generally sceptical. Hot fomentations find 3 supporters; ice 2, one icing the food; 2 use gargles of chlorate of potash; 1 water compresses; 1 the nitrate of silver; and 1 external applications of iodine. Leeches are also mentioned by 1 respondent. The diet and regimen seem to be in America more liberal than in Great Britain, for 3 advocate as much fluid food as can be taken, one of them adding nutritive enemata and free supplies of native wine. The others advocate fluid food. 1 has the food iced. As to the constitution 3 just allude to it as of little importance. One feeds up the strumous, while another gives sulphide of calcium with iron; 1 advocates alkalies in the gouty and rheumatic, and another the iodide of potassium, while a third thinks the hepatic functions should be attended to; as to age and sex only the general rule is alluded to by 3. For the pain in the throat 3 advise ice; 1 incisions; 1 fomentations with iodine; and 1 opium or poppy head fomentations. For the swelling 2 use iodine and belladonna externally. For the pains in the limbs aconite and Dover's powder are recommended by 1; while the advocate of iodide of potassium finds it take care of them. As to constipation 1 uses the citrate of magnesia; 1 saline purgatives; and 1 mineral waters; the others give a general answer. As to surgical interference 4 let out the matter, when an abscess has formed; 1 never uses the knife; and 2 are chary of its use. Scarification is employed by 2 of the respondents. The chief points of difference are that the practitioners of the States do not use guaiac as do the British practitioners; and that the Americans have a more liberal dietary for their quinsy patients than we have.

ARNICA AS A LOCAL APPLICATION.

BY J. FAYRER, C.S.I., M.D.

When deer-stalking in Sutherlandshire during the month of September, I sprained my wrist rather severely, by stumbling in a hole in the peat-moss.

On returning home in the evening I applied a weak solution of tincture of arnica to the injured part, enveloping the lint soaked in the solution with a piece of oiled silk. This was removed after two or three days; the pain of the sprain having abated, and the swelling subsided. The moisture of the application had, meanwhile, caused a few small papules to appear on the skin. These, however, quickly disappeared, and the surface seemed sound.

About six days later I happened to come in contact with a nettle, which stung the wrist very slightly. On the same evening violent irritation of the part set in, and the skin of the wrist and forearm, exactly corresponding to the early application of the arnica, became hot, painful, and affected with an intolerable itching and burning sensation. The following day it was much worse. The skin was much inflamed, and the pain extended up the arm to the axilla, the absorbents being very painful. The skin was red, brawny, and covered with patches of an eczematous character.

Rest, purgatives, and the application of acetate of lead to them gave relief, and the next day the acute symptoms had subsided, but leaving the wrist still swollen, brawny, and painful, with irritating spots of eczema, which gradually dried into scales.

The mischief was confined strictly to the area of application of the arnica, and the result did not entirely disappear for many days. Had I not been in good health at the time, I feel sure that severe erysipelatous inflammation would have resulted; as it was, the symptoms were severe and very painful. It was remarkable that they were limited, by a well-marked line of demarcation, to the part affected by the arnica, so far as the surface of the skin was concerned; and that the nutrition of the skin had been so far compromised that, although apparently perfectly well, and after the lapse of several days, the slight irritation caused by a very trivial sting from a nettle was sufficient to excite so much mischief.

It has been before pointed out that arnica has the property of causing peculiar irritation of the skin. This case not only confirms it, but shows how necessary it is to be cautious in applying it, and in limiting the duration of the application.

THE USE OF LIQUOR BISMUTHI FOR HÆMORRHOIDS AND PROLAPSUS ANI.

BY JOHN CLELAND, M.D., F.R.S.

Professor of Anatomy and Physiology, Queen's College, Galway.

WHILE it may be freely admitted that in many instances hæmorrhoids cannot be treated successfully without surgical operation, and while, for my own part, in a considerable experience I have always had good reason to be satisfied with the results of the operation recommended by Mr. Syme, it is evident that in a large number of cases operative interference is unsuitable; in others the affection, however ameliorated by such interference, remains uncured; and it is desirable that whenever it is possible, a cure should be obtained without resort to so disagreeable a measure. With this end in view I venture to say a few words on the use of liquor bismuthi given as enema.

My attention was first drawn to this remedy by a rather peculiar case of prolapsus of the bowel. A middle-aged woman came for consultation in such a condition that she could with difficulty walk, inasmuch as whenever she parted her thighs, the bowel emerged and hung down for about six inches, in folds of such a character as made it evident that at least half-a-yard of intestine was extruded. The whole surface of the mucous membrane exposed was a deep raspberry red, like those cases of hæmorrhoids which some practitioners delight to treat with nitric acid. This condition was chronic; external supports had failed; the possibility of removal of the whole prolapsed mass suggested itself, but such an operation attended with enormous risk was not to be thought of in the case of a patient enjoying a certain measure of health. Astringents had been tried and failed, and it seemed questionable if astringents were the most suitable remedies in such a case. It appeared much more probable that an irritated and congested condition of the mucous membrane led to a derangement of the action of the muscular walls than that in a strong woman, a local relaxation, involving sphincters and intestinal walls, had produced a prolapsus, which led to congested mucous membrane, from exposure. I recollected the relief frequently obtainable in cases of hæmorrhoids, by application of white bismuth or oxide of zinc. In this case, however, ointment or powder obviously could not be effectually applied. But the liquor bismuthi in stomach affections has a soothing influence far superior to white bismuth. I therefore directed my patient to mix a dessert spoonful of liquor bismuthi with half a wine glassful of starch, and after getting into bed and returning the bowel to its place, to introduce this enema and retain it. I was much pleased, a few weeks afterwards, by my patient calling to tell me that she was nearly well, and to ask if she might continue the remedy. This she was ordered to do; and I have every reason to believe that she has had no return of her malady. I have since frequently used the same remedy for the ordinary prolapsus in children, with invariable and rapid success.

In severe hæmorrhoids there are usually three parts affected, the integument, the mucous membrane, and the hæmorrhoidal veins. Plainly the veins cannot be reached by local medicaments, and those comparatively few cases in which they alone are involved must be treated in other ways. The integument, together with the edge of the mucous membrane up to the grasp of the sphincter, is within easy reach, and may be treated in various ways according to circumstances. Thus, when the congestion is superficial and produces a catarrhal oozing, bathing with whisky or other alcoholic lotion, a small pad of dry cotton wool firmly applied to soak up the moisture, and also zinc or bismuth in powder or ointment are all exceedingly useful; and when a congested surface within easy reach is accompanied with venous engorgement, tincture of iodine sometimes produces surprising effects, although in other instances it is too painful to be borne. But when the mucous membrane is considerably involved, I know no application to compare with injection of liquor bismuthi, which has the advantage of being painless; and, as in the case of prolapsus narrated above, the improvement of the mucous membrane has a wonderful influence on both the veins and integument. In instances in which the necessity for surgical interference appeared indubitable, I have had the gratification of defrauding myself of the pleasure of operating, and of seeing the patient recover. This is the more gratifying, as the surgical treatment of hæmorrhoids labours under the disadvantage, that, no matter what be the particular operation adopted, it never removes the predisposing cause of the malady.

THE PHYSIOLOGICAL ACTION OF ALCOHOL.

BY T. LAUDER BRUNTON, M.D., F.R.S.[1]

Is alcohol a food or a poison? Is it one of the greatest boons ever given to mankind, or one of the greatest curses wherewith they are afflicted? These are questions to which we will receive different answers, according to the circumstances under which they are asked. If we ask the man who has just watched by the bedside of his dearest relation during the crisis of a fever, and seen the parched tongue grow moister, the delirium lessen, the quivering pulse grow stronger and steadier under the influence of alcohol, he will probably tell us that if not a food of the same kind as bread and beef, it is, under certain circumstances, better than either, and a blessing whose greatness can hardly be over-estimated. If, on the other hand, we address ourselves to the squalid wife of a drunken husband, who, instead of employing his time in work, and properly spending his evenings, lies in a state of idleness and incapacity for one half the week, and spends the greater part of the wages he receives for the other half in brutalizing himself at a gin-palace, we shall probably hear that it is the greatest curse upon earth, a poison destroying soul and body; and that but for it she would be a happy woman, instead of a trembling slave, living in constant fear of blows or death, her husband would be a respected member of society instead of a brutal coward, and her home a Paradise instead of a Pandemonium.

If we inquire why people drink it at all, the answers we receive are no less contradictory. The negro sweltering under a tropical sun drinks it to cool himself; the London cabman shivering at his stand on a wintry morning drinks it to warm himself; the weary traveller drinks it to strengthen his flagging muscles, and help him onwards to his destination; the literary man drinks it to give subtlety to his intellect, or brilliancy to his wit; the overworked man of business drinks it to rouse him from his apathy, and give sharpness to his bargains; the gamester quivering with excitement drinks it to steady his trembling hand; and the man or woman broken down by misfortune, and weary of life, drinks it to drown care in temporary oblivion.

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[1] Read in part before the Medical Society of London.

Irreconcilable as these answers to our questions may seem, we nevertheless know that they are all more or less true; and, in studying the physiological action of alcohol, our endeavour must be to discover how it is that one drug can produce such opposite effects. This is undoubtedly a difficult task, and one which we cannot at present hope to accomplish perfectly. All that we can do is to take the facts we find and arrange them to the best of our ability, trusting to future research for information on those points of which we are now ignorant. In doing this we must bear in mind that alcohol has a threefold action. 1st. Its local action on the skin or mucous membrane with which it comes in contact. 2nd. Its reflex action on other organs, through the sensory nerves of the skin or mucous membranes. 3rd. Its action on the brain, spinal cord, and other organs to which it is conveyed by the blood.

The action of alcohol is modified, too, by the degree of concentration in which it is employed, and by the admixture with it of other substances, such as ethers of various kinds, hops, vegetable acids, &c. Thus, if we moisten the skin with pure alcohol, in the form of eau de Cologne, or diluted with its own bulk of water, as brandy, and allow it to dry spontaneously, a decided sensation of cold will be produced; but if we employ it in a still more diluted form, as wine or beer, the cold will be much diminished, or become quite imperceptible. This cooling action is due simply to the volatility of alcohol, which during its evaporation abstracts heat from the skin and cools it down. If pure it evaporates quickly and produces much cold, but if mixed with much water the evaporation of the mixture is too slow to produce any marked result. Any other volatile substance would have a similar effect, although its other actions upon the body might be utterly different from those of alcohol.

And, indeed, we get a very different result from alcohol itself, if, instead of allowing it to evaporate spontaneously, we prevent evaporation altogether by covering the moistened skin with gutta percha tissue. Instead of coolness we get a burning feeling, most intense if we use pure alcohol, or eau de Cologne, less with wine, and imperceptible with beer. We have got rid of the action which alcohol owes to its volatility, and we have brought into play another which it owes to its chemical properties. So long as it could evaporate readily it acted almost entirely on the epidermis, but when evaporation is prevented it soaks through the epithelium and acts on the vascular tissues beneath. This is better seen if, instead of applying the alcohol to the skin, where the epidermis presents a considerable resistance to its passage, we put it into the mouth, where the thinner epithelium offers less obstruction. Almost immediately after its introduction we experience a burning sensation, which increases for a little while, and then gradually diminishes. If we keep it in the mouth long enough, we notice that the mucous membrane changes its character, and becomes whiter, more opaque, and somewhat corrugated. Although the burning feeling appears to be accompanied by an increased flow of blood to the part, and its disappearance by a diminished flow, yet it is not due to the warmth of the blood, for water at a temperature much above that of the blood produces no such feeling in the mouth. Both the sensation of burning, and the visible alteration in the mucous membrane, are due to the action of the alcohol upon the tissues, and we shall better comprehend the nature of this if we compare it with that of other substances. A piece of hot metal, or a solution of corrosive sublimate, will also cause a burning feeling, and an alteration in the mucous membrane, but, instead of being transitory, it will be more or less permanent. Now there is one point in which they all agree, viz. they all coagulate albumen; and the whitened appearance of the mucous membrane of the mouth after brandy has been long applied to it, is no doubt due to the precipitated albumen on the surface obscuring the red colour which the circulating blood imparts to the tissues beneath. But there is this great difference between the action of alcohol and that of heat, or of corrosive sublimate. The latter produce permanent coagulation, while the coagulum formed by alcohol readily dissolves again in water, or in the liquids of the body.[1] Thus its action is more transient; and, if it is only allowed to act for a short time, its effect is counteracted by the blood which dissolves the albumen as fast as it is coagulated, so that we do not see any opacity of the mucous membrane of the mouth, unless alcohol has been acting on it for a good while. When frequently applied to the skin, and allowed to evaporate, it seems to act on the epithelium and harden it, and thus it is frequently used to prevent the formation of bedsores and cracked nipples.

Most substances which possess the power of coagulating albumen, such as tannin, catechu, kino, logwood, sulphate of copper, sulphate of zinc, &c., act as astringents when taken internally, and even corrosive sublimate although not usually reckoned among their number is strongly recommended in some forms of diarrhœa by Professor Sydney Ringer. Alcohol is no exception to the rule, and we all know that a person suffering from an attack of diarrhœa usually flies to the brandy bottle for relief before he thinks of consulting a medical man. We know too little about the action of astringents to say positively that alcohol owes its efficacy in this respect to its power of coagulating albumen, but we certainly can say that this properly appears to be the only one it possesses in common with catechu and kino.

The simple experiment of putting a little brandy in the mouth is instructive not only by showing us the local changes which alcohol produces in the mucous membrane, but by reminding us of the second kind of action which alcohol exerts, viz. reflexly through the nervous system. At the same time that the burning is felt, the saliva begins to flow copiously into the mouth. The alcohol has not come in contact with the salivary glands at all, but through the sensory nerves of the mouth it has acted on the nervous centres and through them upon the vessels and secreting cells of the gland. If we swallow the brandy instead of ejecting it, the feeling of warmth which we experience along the course of the œsophagus and in the stomach itself, informs us that it acts on the mucous membrane of these organs in the same way as on that of the mouth. So far as I am aware, we are at present ignorant of the reflex actions which alcohol exerts through the mucous membrane of the œsophagus, but those which it produces through the stomach are of great importance. First of all come those which concern the secretions and movements of the stomach itself. When the stomach is empty its mucous membrane as seen through a gastric fistula is pale and only covered with a little mucus. If a little alcohol is now introduced the blood vessels of the mucous membrane dilate and it becomes of a rosy red colour, its glands begin to secrete copiously, beads of gastric juice stand upon its surface, become larger and larger until they can no longer preserve their form, when they coalesce and run down together in a little stream.

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[1] The coagulation of albuminous fluids by alcohol seems due in the first instance to the simple abstraction of water, and when this is again added, they re-dissolve. If the alcohol acts for a long time upon them, however their constitution seems to undergo a change, and they become insoluble in water.

Now every slight stimulation of the stomach seems to be felt as appetite, and thus we find that substances having the most diverse properties induce a desire for food. Alcohol does this in a marked degree and a nip of brandy is very frequently taken as an appetizer. But appetite seems really to be only an expression of slight uneasiness on the part of the stomach. It cannot distinguish sensations like the mouth, and alcohol which on the tongue causes burning, quassia which causes bitterness, and minute doses of arsenic or tartar emetic which would cause congestion if they stayed in the mouth as they do in the stomach, all cause appetite. Perhaps they do this only by exciting a certain amount of congestion in it, for food itself causes the vessels of the mucous membrane to dilate and its glands to secrete in the same way as we have described after the injection of alcohol, and we all know that a person who begins a meal with no appetite at all often eats with zest after the first bite, and finishes with astonishment at the amount he has consumed. But if the irritation is too strong the whole condition becomes changed.[1] The mucous membrane loses its rosy hue and becomes pale, the secretion of gastric juice ceases while that of a slimy mucus is increased, appetite disappears and is replaced by nausea, and finally vomiting occurs. This change is often clearly seen in a so-called bilious attack, where the irritation of the stomach first manifests itself as an abnormal craving for food, which gives place as the irritation increases to nausea and vomiting.

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[1] Bernard *Archive d'Anat. Gén. et de Physiologie*, Jan. 1846, quoted by Power; and Carpenter's *Physiology*. Ed. p. 149.

Now the amount of irritation necessary to produce these totally different conditions of increased secretion with appetite and diminished secretion with nausea varies in different stomachs, and in the same stomach under different conditions. If the stomach is sensitive an irritation will cause nausea which would only produce appetite if it were less irritable, and *vice versâ*, a sluggish stomach will be benefited by an amount of irritation which one normally sensitive could not bear. Thus we know that in some cases articles of food, such as lobster, which in normal stomachs frequently cause indigestion and nausea, are readily digested while ordinary food is not digested. The stimulus which an ordinary diet gives to the stomach seems here to be insufficient to excite the secretion of gastric juice, while the more irritating substances do so and are digested instead of causing over-irritation and vomiting as in the normal condition. But if this explanation be correct, how is it that we take a glass of spirits with our lobster "to dige[illegible]" Is not this adding fuel to fire and increasing the irritating effect of the lobster on the stomach by that of the alcohol? By no means —the fibres of lobster are probably in themselves no more irritating than fibres of beef, but only less soluble in gastric juice, so that they retain their form and hardness instead of being reduced to a pulp, and by thus exerting for a longer time a mechanical irritating action upon the stomach they produce nausea and indigestion, not immediately after they have been swallowed, but in the course of some hours. If, however, an increased secretion of gastric juice be produced by means of a glass of spirits swallowed at the same time with the lobster, we may expect that digestion will take place more rapidly, the fibres will be dissolved, and the prolapsed irritation of the stomach being avoided no nausea will ensue.

If insufficient stimulation of the stomach then does not induce a flow of gastric juice and if excessive stimulation causes nausea, under what circumstances is alcohol likely to be useful? Healthy stomachs with ordinary food do not require it, although in small quantities it may do little harm, and as an adjunct to lobster may be positively beneficial. A large quantity however is certain to be injurious. Moreover if regularly used, even in small quantities, the stomach may become habituated to it, and refuse to respond to the stimulus of food alone, unless supported by that of alcohol. The case is different when we have to deal with a stomach whose sensibility is below par, either permanently or temporarily. In patients convalescent from an acute illness, or weak delicate anæmic persons, the food does not sufficiently stimulate the weakened stomach, the secretion of gastric juice is small, and the meal lies for a long time like a weight at the epigastrium. The same is the case with the merchant, the lawyer, or the doctor, who comes home from his counting-house, his office, or his rounds, and sinks exhausted into his easy chair, weary and worn out by a long day's work. In such cases the diminished sensibility of the stomach must be compensated by an extra stimulus, and the glass of sherry which to a healthy person not exhausted by over fatigue would be superfluous, will in them restore the normal equilibrium and quicken the otherwise slow and imperfect digestion.

I do not [illegible]n to discuss the wisdom of these men's conduct in thus exhausting their energies, or the question how long such a course can be pursued [illegible]ut ending in an utter breakdown, for it is in many i[illegible]heer necessity which drives them to it, and no remon[illegible]warning is of any use. But I would say a word about the amount of stimulants to be employed and the probable effect of excess on the stomach itself. Not only does the sensibility of the mucous membrane become blunted so that it no longer secretes gastric juice in proper quantities when stimulated by food alone, but it secretes mucus in large quantities, and this not only impedes digestion, but facilitates fermentation, by which various injurious substances are formed. Amongst these may be mentioned butyric acid which causes an acrid burning sensation in the stomach itself, and may according to Otto Weber and Senator be absorbed into the blood and there act as a nerve poison, still farther reducing the business capacities of the unfortunate patient, which may already have been sadly diminished by overwork and inability to assimilate proper nourishment.

While then it may be very beneficial to take a moderate quantity of alcohol with meals, an excessive amount will be injurious to the stomach itself not to mention its action on the nervous system.

In connection with this power of the stomach to adapt itself to the stimulus it ordinarily receives, I may mention that in one part of the Austrian empire the peasantry live almost exclusively on a mixture of oatmeal and water which is allowed to ferment and become sour. Although this diet would in all probability so irritate the alimentary canal of any ordinary person as to produce vomiting and diarrhœa, these people thrive upon it and are very strong and healthy. When the young men however enter the Emperor's army, and come to Vienna, where they get well-cooked food, they nearly all suffer from indigestion, lose flesh, and become weak and ailing—exactly the contrary of what one would have expected. Of course various explanations may be given of this fact, but I am inclined to believe that the indigestion is due to the well-cooked food being less irritating than the sour meal, and not sufficiently stimulating to the stomach and intestines accustomed to the other.

Alcohol taken into the stomach increases the movements of the organ as well as its secretion, and by mixing its contents more thoroughly with the gastric juice accelerates digestion. At the same time it causes the expulsion of gases, and a little brandy is one of the carminatives most commonly employed by those who suffer from flatulence. But in this respect also the stomach after a little while becomes accustomed to the stimulus, and those habituated to the excessive use of alcohol not only suffer from flatulence due to the processes of fermentation already mentioned, but are less readily relieved by the usual remedies.

(*To be continued.*)

Reviews.

A Practical Treatise on the Diseases of the Eye. Third Edition, Re-written and Enlarged, with 5 Plain and 3 Coloured Plates, and 293 Wood Engravings. By HAYNES WALTON, F.R.C.S., Surgeon to St. Mary's Hospital, Surgeon in charge of Ophthalmic Department of the same, and Lecturer on Ophthalmology, Member of Council of the Royal College of Surgeons of England, &c. London: J. and A. Churchill, New Burlington-street.

THIS is more than a new edition of a well-known work, since the former ones treated only of the surgical disease of the eye. It is a new book, because chapters which embraced subjects before written on, have been re-written, and all the several topics legitimately included under eye-medicine and surgery, are now introduced and fully discussed. Here we acconnt for the portly volume.

The first thing which strikes us in our glance through the book, is the force, conciseness, and perspicuity of the style, and the easy division and tabulation of the subject-matter. These characteristics which render note-taking easy for the beginner, of course render the writing happy for the learner.

We next remark with pleasure that many ophthalmic matters, not sufficiently dwelt on in modern treatises—and some of which are not even mentioned—are freely put forth. Among these are entozoa in and about the eye; diseases of the orbit, and orbital injuries within and without; tumours in connection with the eye; orbital aneurism, and erectile vascular tumours in connection with the eye; injuries from mechanical and chemical agents; wounds of all kinds. Nowhere else is there to be found so much which is required, and which is so thoroughly practical.

The chapter on cataract is a long and valuable one; some of the views so strongly held by what may be called the German school on certain methods of extracting cataract are not favourably entertained by our author. He is not alone in his opinions. The optical defects of the eye and the ophthalmic department, which may be called the new ophthalmic subjects, are sufficiently considered. We meet with passages in different chapters which we might be more willing to discuss than to receive; but perhaps after all, they relate more to matters of opinion than to matters of fact. But we must stop. The illustrations of the book, the wood-cuts, and the chromo-lithographs, have not been equalled in any ophthalmic work. To those who expect to grind up ophthalmology for examinations in a few days, Mr. Walton's work is of no use. To those who wish to read leisurely on eye-subjects, and who wish to possess a book of reference, the treatise before us can be thoroughly recommended.

Lessons on Prescriptions and Prescribing. By W. HANDSEL GRIFFITHS, Ph. D. Pp. 150. London: Macmillan and Co.

Commentary on the British Pharmacopœia. By WALTER J. SMITH, M.D. 8vo. Pp. 766. London: Smith, Elder and Co.

BOTH these books, written by teachers in Irish medical schools, are deserving of commendation. Dr. Griffiths's work not only gives to the student in clear and concise language directions how to write a prescription, but it contains several hints on the mode of prescribing several important medicines, and of avoiding incompatibles, which may be useful to the practitioner. We do not altogether agree with his dictum that pills should never be given to young children, for sometimes this is the most convenient form of administration, and by cutting the pill into a number of parts and imbedding them in a soft, somewhat adhesive vehicle, such as oatmeal-porridge, they can be given without the least difficulty. This however is quite apart from the main object of the book, from which we think the student will learn how to prescribe as easily or more easily than from any other work of the kind with which we are acquainted.

As its name indicates, Dr. Smith's work takes up the articles of the *British Pharmacopœia* in order, and gives their origin, characters, and medical uses. By means of an ingenious system of genealogical tables, the derivation and family connections of many of the preparations of the inorganic materia medica have been made much more easy to recollect. The tests for the recognition of drugs and the detection of adulterations are also clearly given. The parts treating of the medicinal applications have been less carefully done than the chemistry of the drugs, and until we read the derivation given by Dr. Smith in p. 191 of the word assassin, we were under the impression that it came from a certain mediæval sect, among which hashish was used for a definite purpose, and not from the general tendency of the drug to render persons who take it furious and even murderous. Even in this part of the book, however, much useful information has been put in a small compass, and any slight slips such as the one we have quoted do not seriously detract from the value of the book.

Principes de Thérapeutique Générale. By PROFESSOR J. B. FONSSAGRIVES, of Montpellier. 8vo. Pp. 472. Paris: Baillière et Fils. London: Baillière, Tindall and Cox.

WE cannot but welcome anything that comes from the pen of one who has such a widely spread and deservedly gained reputation as Monsieur Fonssagrives, and the present work bears evidence, as we would naturally expect, of his long and earnest study of therapeutics. It does not deal with the action of separate drugs, but with the general questions which refer to all remedies. After a well-aimed blow at pure expectancy in medicine he attacks polypharmacy both under its ancient form of prescriptions containing a multitude of drugs, and in the form in which it now more frequently shows itself of the practitioner prescribing first one medicine and then another, so that they follow like the courses of a meal until a great part of the pharmacopœia has been gone through, and the patient's constitution can neither proceed freely with its curative measures nor bring to a close its morbid processes. The absorption of remedies by various channels, their circulation in the blood, apparent affinity for certain organs, chemical changes and elimination are then discussed, as well as tolerance and intolerance, effect and habit, and cumulative action. The next two chapters are devoted to the general action of drugs on the circulation, animal heat, respiration, nutrition, and nervous system, and to their local action and general effects. Although the author is evidently well acquainted both with English and German literature, we miss the name of Gee in his notice of apomorphia, and in his description of the action of drugs on the tissues at p. 183, we find no reference to the classical work of Voit and Bauer on phosphorus, to which we owe so much insight into the processes of tissue change within the organism. The next two chapters are devoted to the variability of medicinal actions and the mechanism of the curative action of drugs, and then comes a discussion of the methods of testing their action and value. We are inclined to attribute fully more importance than M. Fonssagrives to the results obtained by experimentation on animals, but we perfectly agree with his remarks on the trials of drugs on man, and recommend them to the perusal of every one interested in the subject of therapeutics. In the chapter on the time for administering medicines, the author remarks that the American proverb Time is money, ought to be altered to Time is life; and among other interesting observations on this subject, he says that the age as well as the temperament of the doctor will influence the time of his interference, and while the young man will be in too great a hurry, and the old one too slow and disposed to tarry, the middle-aged one will be more likely to strike the happy medium. The author then gives a short history and criticism of the various classifications of remedies which have been proposed, and then gives one of his own based on the very sensible plan of trying to arrange the drugs according to the use for which they are wanted, in the same way as one arranges surgical instruments in a drawer. He casually mentions that for several years he has been engaged on a work which will give their indications and uses and to which the present one may in some degree be regarded as an introduction. We shall await its appearance with impatience, and in the meantime recommend the present one to our readers' perusal.

Journal of Anatomy and Physiology. Conducted by G. M. HUMPHREY, M.D., F.R.S., WILLIAM TURNER, M.B., MICHAEL FOSTER, M.D., F.R.S., AND WILLIAM RUTHERFORD, M.D. Vol. X. Part I. 8vo. Pp. 222. London and Cambridge: Macmillan and Co.

WE welcome it as a sign of the progress which the branches of science to which this journal is devoted are making in this country, that instead of appearing half-yearly as hitherto it will henceforth be issued quarterly. Under the able editorship of Professors Humphrey and Turner it has gained an honourable place amongst the scientific literature of the day, and is even better known and appreciated on the Continent than in this country. Professors Foster and Rutherford, who have now become associated in the editorship, will superintend more especially the physiological department, and the present number gives promise that it will maintain its character under the altered management, although strangely enough its contents are more anatomical than they usually were when anatomists only were the editors. The two most interesting papers to medical men are by Francis Darwin, M.B., "On the Primary Vascular Dilatation in Acute Inflammation," and by J. N. Langley, B.A., "On the Action of Jaborandi on the Heart." The former paper ought to be read with care by all who are interested in the subject of inflammation, and the latter is interesting not only on account of the importance of the drug experimented on, but because it leads to some conclusions regarding the nature of the cardiac ganglia and the action of drugs upon them different from those which are generally held.

Clinic of the Month.

Caustics in the Treatment of Diseases of the Neck of the Uterus.—Dr. Tilt, in speaking of the efficacy of various forms of caustic in the treatment of diseases of the neck of the uterus, sums up the results of his observations in the following remarks:—1. That, in comparatively recent cases of endocervicitis, nitrate of silver, tincture of iodine, or carbolic acid, suffices. 2. That chronic cases of endocervicitis had best be treated by acid nitrate of mercury or nitric acid. 3. That hyperchronic endocervicitis with considerable cervical hypertrophy requires potassa fusa cum calce, or some strong acid. He thinks Dr. Braithwaite is too general in his recommendation of nitrate of silver. (*British Medical Journal*, Dec. 1875).

The Treatment of Phagedenic Gangrenous Venereal Sores.—D. B. Simmons, M.D. of the Ken Hospital, Yokohama, Japan, remarks that few who have had any considerable private or hospital practice are unacquainted with the difficulties often experienced in arresting the destructive progress of what are known as phagedenic gangrenous venereal sores.

Within the last two years several cases which will admit of this classification have come into his hospital for treatment. The hot iron, nitric, chromic, and carbolic acids were all tried in turn, as well as "Ricord's born enemy of phagedena," with what was probably the average rate of success.

The last four cases, and one especially, which he refers to, were almost entirely treated, however, by, if not a new process, one which was productive of such satisfactory results as to warrant him in earnestly recommending its trial. *This consisted in the continuous immersion of the diseased part in hot or warm water.* One of the cases referred to had been treated, before falling into his hands, by the usual means for nearly two weeks, and though not progressing so rapidly as at first, was still advancing. Both labia, minora and majora, the fourchette, clitoris, and portions of the urethra had disappeared, and all the region occupied by the external organs of generation had been converted into an immense irregular cavity, discharging an unhealthy, sanious, and very fetid pus. The patient was also suffering much pain, especially when the parts became in the least dry. A sitz-bath of the ordinary size and form was required. In this was placed a cushion, or large bathing-sponge, to render more tolerant the long-continued position in the tub, required for obtaining the more satisfactory results. In the case referred to, the matter seemed to be increased by the use of the bath for thirty-six hours, when it began to change in character. Instead of the sanguino-serous pus which was discharged from it before, it had assumed a more healthy, "laudable" form. The ragged edges sloughed off, and their dark-red or purple colour presented a bleached-out appearance. From this time the progress of the disease appeared to have been arrested.

Through the dirty gray slough at the bottom granulations began to appear, and the healing up of the part, as far as the extensive loss of substance would admit, was completed in two or three weeks. Uncomfortable as the position was, the patient would not remain out of the bath for a moment longer than necessary, on account of the great relief it afforded from the burning, smarting pain.

Dr. Simmons found, after the change had commenced for the better, that it was not necessary to keep the patient so continuously in the bath, but only on alternate hours. In the interval, iodoform was sprinkled freely over the part. It is his opinion that the destructive agency is to be found in the peculiar or specific character of the discharge, and that the water simply removes or dilutes it, so as to destroy its action, the same as it would with a caustic. He does not see why this explanation is not as true and satisfactory as that the matter from a soft chancre has specific qualities, and is capable of reproducing itself, under favourable circumstances, to almost any extent.

A very bad case of multiple chancres in a female, of an inflammatory type, was treated by him in the same manner, with the same satisfactory results, about the same time.

In the male, when the organ is sufficiently large or long, he has found its immersion in a tumbler or cup of water answer the same purpose.

He now adopts this plan in the treatment of all soft chancres that are troublesome, and is satisfied that if it is properly carried out they may often be cut materially short in their duration.

All are acquainted with the suddenness with which soft chancres often get well after long and varied modes of treatment. Dr. Simmons believes this to be consequent upon a change in the character of the discharge, or in its irritating corrosive action, which allows of the natural healing of the wound. The same is practically effected by the dilution or removal of it by the water.

Irrigation, he has no doubt, would answer the same purpose, and could be arranged so as to subject the patient to far less inconvenience and discomfort.

He thinks, however, that the water should be at as high a temperature as possible in either case. (*The Medical Record*, No. 253, 1875.)

Irritability of the Female Bladder of fifteen years' standing cured by Dilatation of the Urethra and Neck of the Bladder.—Mr. H. B. Hewetson observes:—The notes of this case are of extreme importance in connection with an operation, still on its trial, introduced by Mr. Pridgin Teale, and but recently published, which comprises dilatation of the urethra in the female for the relief of irritability of the bladder, and, as in this case, occasional retention of urine. The absolute success attendant upon its performance in some cases, and its partial, if not complete, failure in others, render it incumbent that strict records of the symptoms, general as well as local, which affect females, the subjects of irritable bladder upon whom this operation is performed, should be kept. The extent to which the dilatation of the urethra is carried should especially be noted, since the partial or complete failures may possibly be the result of too cautious stretching from fear of producing incontinence of urine. Moreover, the general symptoms must be taken into account, for it may be that it is upon them the surgeon is consulted, without the slightest reference being made (as this case will show) to the more delicate point of irritability of the bladder; when it is upon the latter trouble that the general malaise depends.

The following are the notes of the case:—Miss M——, aged thirty-six, sent for Mr. Hewetson on the night of March 3rd, 1875. On arrival at her home, he found her to be suffering intensely from retention of urine. He relieved her (by the use of the catheter) of a large quantity of urine—such an amount as must have distended the bladder nearly if not quite up to the umbilicus. This point he did not test, being anxious to relieve her without delay from the worst agony of retention. The retention returned in a day or two, and he had again recourse to the use of the catheter.

Her previous history is as follows:—That she was a perfectly strong and robust woman, following the *arduous* duties of a "present day" schoolmistress until fifteen years ago, when she was seized with an inflammation of the bladder, for such it was termed by her medical attendant. This was followed by the formation of a small abscess in the region of the urethra, which discharged of itself. Since that time her health has been bad, her nights have been wakeful and disturbed by being constantly "every half hour or hour" obliged to get up to pass small quantities of urine with great effort and some pain. She is low and depressed, with headache, loss of appetite, and continual bearing down, resulting in a total unfitness for prolonged exertion of any kind. She has lost flesh considerably.

On examination Mr. Hewetson found the orifice of the urethra to be completely surrounded by warty growths of considerable size, and on examining the rectum the introduction of the finger was impeded by a very tight sphincter ani. The rectum was baggy, and there was a small external pile. The uterus was in its natural position, and the catamenia were and had always been, regular, and the urine was natural. Not being able to estimate to what extent the retention was due to the warty growths, and seeing she was suffering considerably from rectal difficulties, he deemed it advisable to negative the possibility of the retention being caused by the warty growths by removing them, and whilst the patient was under the influence of an anæsthetic stretching the sphincter ani with the forefingers introduced back to back sufficiently forcibly to paralyse it for a time and allow the sore produced by the snipping off of the pile to heal in the same way as one would cure a fissure of the anus, by setting the spasm of the sphincter ani at rest, which, constantly contracting, might possibly be an element, through reflex action, in helping to keep up the vesical irritability.

Accordingly, on March 5th, chloroform having been administered, Mr. Hewetson completely carried out the above suggestions. The result of this was, that during the next few weeks relief was given to the retention of urine and to the pain in passing the motions; but there was no relief at all from the vesical irritability. Her general condition, with these exceptions, continued as before, and there was a return of the retention of urine at the end of three weeks from the operation.

Mr. Hewetson had told her a second operation would possibly be necessary should the first fail to give relief, and accordingly, on April 11th, he again placed her under the influence of chloroform, and introducing Weiss's female dilator into the urethra to the extent of about two inches, he then *slowly* separated the blades of the dilator, stretching the urethra so as to admit of the introduction of the forefingers within the bladder while the parts were on the stretch. On closing the blades and withdrawing the instrument, the urethra contracted upon the little finger, so as sensibly to grip it when introduced into the bladder, the coats of which were thickened. There was no foreign body or stone to be detected.

She was very much upset by the chloroform-sickness, which continued more or less all night; there was, however, *no more irritability of the bladder, no retention, no incontinence produced,* and, to use her own words, "I have not passed water so freely for years;" nor had she retained it so long without being disturbed; for the first time she made water was in the evening after the operation, and she was not disturbed during the whole night.

April 14th.—She slept the night through, and awoke with little or no headache, retaining and passing her water quite naturally with the exception of some soreness.

Her progress now continued to be satisfactory and very rapid.

May 4th.—She reports her old symptoms to have all disappeared. She sleeps and eats well, and takes a fair amount of exercise.

At this stage Mr. Hewetson ordered her into the country, where she resides, and returning on May 31st, she reported herself as perfectly well, and as having gained three stones in weight. On the day previously she had walked a distance of eight miles without feeling more than ordinary fatigue, and remarked that before the operation "she could hardly trail herself about."

Thus, then, were the miserable and intractable sufferings of years, shutting out this poor woman alike from society and employment, put an end to at once by an operation whose best recommendation is its simplicity and its success. (*Lancet*, Dec., 1875.)

The Rapid Relief of Neuralgic Pain.—Dr. Spencer Thomson considers that instead of the employment, as of old, of external applications to palliate suffering in neuralgic affections, employed with the hope that the disease might be conquered after a longer or shorter interval with quinine, carbonate of iron, arsenic, and other antiperiodics which were not always certain, we may now congratulate ourselves that a large number of cases of so-called neuralgia may be quickly, either permanently cured by the relieving remedy, such as phosphorus, or relieved, until such time as specially curative agents, or curative general treatment, have removed the tendency to the recurrence of the pain. As one of the newest of the remedies he first alludes to one which is much too slowly making its way into the domain of practical therapeutics—the recently introduced "tincture of gelseminum sempervirens," or yellow jasmine. In his presidential address delivered before the South-Western Branch of the British Medical Association in 1874, he alluded to this remedy as having proved very successful in his hands, and in a paper he read this year at the Plymouth meeting he was able to state how fully it had fulfilled his expectations during the twelve months that had elapsed since his former notice of it. Directly or indirectly it had been used by him, or by his authority, in at least forty cases to which it was applicable, and with almost constant success. In using the word "applicable," he does so in accordance with his own experience that the remedial power of the gelseminum seems confined to those branches of the trifacial nerve supplying the upper and lower jaw, more particularly the latter, and more especially when in either jaw the pain is most directly referred to the teeth or alveoli; indeed, he can scarcely recall an instance of the above in which relief was not speedily and thoroughly given. The usual expression of the patient has been, "It acted like a charm." In illustration he gives one case.

On Sunday afternoon, June 20th, the housemaid of a friend, a retired medical man, came to him with a note from her master, stating that she had been suffering from agonizing pain, of what was thought to be toothache, for six-and-thirty hours. Nothing gave relief, and no dentist could be found to remove the only suspicious tooth. He sent her home with a bottle of gelseminum tincture, which he kept for home use, desiring that she should have twenty minims at once, and twenty more within two hours if not relieved. Her master sent him a note in which he stated that the patient had experienced immediate relief.

In the notices he has met with on the use of the gelseminum, the doses quoted seem all too small. Dr. Thomson now almost invariably prescribes for an adult twenty minims of the tincture as a first dose, to be repeated any time after an hour and a half if relief is not given. He has rarely had to order a third dose, and he has never found any inconvenience result from the larger doses. In one instance, a gentleman who, unadvisedly, took thirty minims at once, and immediately afterwards went out driving, told him he experienced for an hour or two some uncertainty of vision when guiding his horse. A severe attack of neuralgia of the jaw was, however, cured by the one dose, and did not return.

After referring to the solution of Calabar bean, to the tincture of actæa racemosa, and the nitrite of amyl, Dr. Thomson concludes by saying that, with morphia, and occasionally—but only occasionally—atropia, to use hypodermically, with phosphorus in solution, with gelseminum, aided at times by the ordinary external appliances, such as heat, or freezing if need be, aconite, and chloroform, one ought to feel fully prepared to meet and subdue at the time most cases of neuralgic pain, and, indeed, of pain generally. (*Lancet*, December 1875.)

Extracts from British and Foreign Journals.

Treatment of Diarrhœa in Young Children.—After referring generally to the symptoms of diarrhœa in children under two years of age, Dr. Oliver remarks that the presence of undigested food in the dejections of a young child, especially if that child exhibits evident marks of deficient nutrition, is an indication that the diet is not suitable, and that it should be changed. The accurate adaptation of diet is by no means an easy task in such cases; children at the breast, and under good hygienic influences are not usually affected with this disease; articles of food from which a healthy child derives his principal support will here often fail altogether; even milk must sometimes be dispensed with; up to a certain time farinaceous food should be given with the greatest care. It is not uncommon to find cases where milk, whether diluted with water or thickened with isinglass, or farinaceous food, cannot be digested. The pale, putty-like matter of which the dejections consist and which is passed in such large quantities, is evidently dependent upon the milk diet, and resists all treatment as long as that is continued. The isinglass and milk above referred to was first introduced by Dr. Meigs, of Philadelphia. The proportion of gelatine, is about one teaspoonful, to be dissolved in water, and mixed with half a pint of milk. Liebig's soup is tolerably well borne in many cases. It is well known that flour is partially incapable of digestion in the stomachs of infants, whilst it is equally well known that at a later period the power to transform starch into sugar, and thus digest it, is increased. For twenty years Dr. Van Wych, of San Francisco says he has discountenanced the use of diluted cow's milk, substituting properly diluted and sweetened fresh cream, solely on the ground that a nearer approximation to woman's milk can be effected, than in any other way known to him, and hence there is less liability to produce injurious effects. Apart from this there are good reasons for using only cream which rises after the milk has stood some twenty-four hours. Should the food prove too strong it will be necessary to make a further dilution, with an alkali, a certain amount of lime water, or bi-carbonate of potassa. If the child is no more than six months old nothing should be allowed but milk, or some preparation of milk. If the child be very ill one should beware of feeding too often, particularly if farinaceous foods enter largely into the diet. If it excite flatulency, or if any sour smell be noticed from the breath or evacuations, the quantity of food should be diminished. Beyond the age of six months beef tea, raw beef, or the yolk of an unboiled egg, may be added to the diet. The egg is best digested when beaten up with a few drops of brandy. If the child be so ill that he takes but a small quantity at a time, it may be well to give beef tea (made in a bottle, or the juice of a steak slightly broiled and squeezed in a lemon squeezer) in conjunction with milk. A teaspoonful may be given occasionally; and its digestion will be aided by adding a pinch of saccharated pepsine to each teaspoonful of the juice or tea. If the stools are loose and are passed frequently, two or three grains of subnitrate of bismuth may be given, and if much straining be noticed a drop of tinct. opii. or a little Dover's powder, will be a useful addition to check the abnormal briskness of peristaltic action.

Enemata of the fluid extract of Krameria, containing tinct. opii are occasionally useful. If an aperient is required there is nothing better than castor oil, or the tinct. rhei. aq. of the German Pharmacopœia. The latter is an alkaline tincture, and is an exceedingly useful preparation.

So long as the tongue remains furred, or the dejections acid (to litmus paper), the alkali should be persisted in, and the aperient may be repeated every third morning. Alteratives (calomel, &c.) are in these cases of little value. It is useless to stimulate the functions of the liver. Under the use of acetic acid, aromatics, &c., food soon begins to be digested, and the stools become more healthy. As soon as convalescence is fairly established, iron and cod-liver oil may be given. To ascertain whether the child is really gaining it will be well to have him weighed once a week. This treatment, when reviewed, may to some seem like hypermedication, but all that is advised is in reality food, with the exception of the bismuth powder and the aromatic.

The alkali, pepsine, and stimulant, are no more medicine than milk, barley, and beef tea. In all these cases where a special diet is recommended, a dietary should be written out by the medical attendant—the kind, quantity, and time for each meal specified; as it is often upon the judicious arrangement of food that the recovery of the child depends, and when the diet is properly directed the exact medicine to be ordered becomes a matter of secondary importance comparatively.

After an attack of diarrhœa acid preparations are advisable, such as pernitrate of iron with dilute nitric acid, in conjunction with cod-liver oil or the sulphate of iron gr. ss. to gr. ii. with sugar, and given in ginger wine. If there is a tendency to constipation after diarrhœa iron, in the form of the tartrate of potash and iron, is preferable. A point which must not be overlooked is attention to the action of the skin. In these cases the cutaneous secretion is apt to be suppressed early, and the skin becomes dry, rough, and harsh; when this is the case the child must be bathed every evening with hot water, and anointed with warm olive oil and warm clothing, and swathed in flannel. (*Boston Medical and Surgical Journal*, July 8, 1875).

Alterations of Sensibility in Articular Rheumatism, and the Electro-Therapeutical Treatment of the Disease.—Dr. Drosdoff has called attention to a very remarkable reduction of sensibility to the action of the electric current that he observed in a case of acute articular rheumatism, though both the pain in movement and when pressure was made were very considerable, from which he concludes that there are separate channels for the conduction of different kinds of stimuli, and that the perception of various kinds of sensation should be more carefully investigated in other cases. For faradisation he recommends the use of Dubois Reymond's apparatus with Grove's elements. The feeling of space he tests by means of Weber's circle method, and the perception of pressure by Wright's plan. The sensation of temperature may be tested by means of metal plates dipped in water at different temperatures. In the case he records, the application of very severe shocks of electricity was not perceived, though lively pain was experienced on the slightest pressure. It was curious to observe that the special diminution of perceptivity was strictly limited to the parts affected with the disease—just beyond these parts the sensibility was as acute as elsewhere. The reduction of sensibility for electric excitation begins he thinks earlier, and remains longer than the pains in the joints, and there is no prospect of permanent recovery as long as the electro-cutaneous excitability has not returned to its normal amount. Dr. Drosdoff observed, that coincidently with the diminution of the electro-cutaneous sensibility, the faculty of perceiving pressure was diminished, whilst that of perceiving variations of temperature was exalted. The tactile sensibility is augmented in acuteness, but also in many cases undergoes abnormal variations. The increased thermic and tactile sensibility is diminished after from 5 to 10 minutes persistent faradisation.

The cutaneous temperature over the affected joints is constantly two or three degrees Centigrade higher than in the parts in the vicinity. This exaltation of temperature, as a rule, precedes rheumatic pains, and lasts after they have departed. It also is reduced by faradisation applied for a few minutes, and the reduction lasts for several hours. The application of faradisation does not protect the patient from a relapse, but it materially shortens the duration of an attack and renders it much milder. Drosdoff consequently strongly recommends that it should be employed as one of the means in the treatment of acute articular rheumatism. (*Botkin's Klinik zu St. Petersburg und Rundschau,* Oct. and Nov. 1875.)

Electricity, its Therapeutic Applications.—The electric force in all forms is a disturbing element, and is therefore unsuited to the conditions in which *rest* is the indication.

It may be assumed that this agent is inapplicable in all cases of recent or acute inflammation. After the subsidence of the acute symptoms, however, this agent may be beneficial. The same is true of friction and vibration. The appreciation of this principle will save some disappointments from the misapplication of the remedy.

Headache from acute inflammation or acute congestion will, according to this principle, be aggravated by electricity. A headache from inanition or anæmia will be benefited, and sometimes instantly removed by electricity. A sympathetic headache will be removed by this agent when the sympathetic cause is removed.

A dyspeptic headache will generally prove intractable, because the gastric derangement will defy removal by this agency.

On the other hand, a headache dependent upon defective innervation in the sympathetic, or ganglionic system, will disappear by the electric stimulation of the sympathetic ganglia. In this case it is not necessary to include the brain within the scope of the current.

In the chest "a stitch in the side," or any form of neuralgia is generally benefited, while a true pleurisy is made worse. In doubtful cases, therefore, the behaviour of the agent may be made a means of diagnosis.

In the abdomen, constipation from sluggish muscular action and deficient intestinal secretion is benefited by this agent, while the same symptoms resulting from inflammatory action are only made more intense. The intestinal muscles paralyzed by inflammatory congestion are still more congested by the electric stimulus.

In the loins, the condition of lumbago is favourable for the beneficial action of this agent, while a deep-seated inflammation is unfavourable.

In the pelvis, especially in the female, the organs are benefited or injured, according as we comply with this principle, or neglect, or misunderstand it.

A congestion dependent upon local muscular passiveness in the walls of the vessels ought to be benefited, while a true acute or recent inflammation will be aggravated. The practical appreciation of this agent should upon this basis be an aid to diagnosis.

Sciatica follows the rule of lumbago, while an inflammation of a nerve presents a condition unfavourable for the application of electricity.

An exception may be made in the apparent effects of a powerful galvanic current which is capable of producing a benumbing effect, even in an inflammatory condition. In this case, the apparent benefit is temporary, the pain soon becoming as bad as before.

Modes of Action of the Different Kinds of Electricity.—The galvanic current presents as its prominent effect electrolysis; the faradic current, stimulation of motion, and sensation. Galvanism may therefore be compared in its effects with those medicines which, like mercury and iodine, alter the nutrition of organs. The current passing through an enlarged lymphatic gland tends to change its nutrition primarily. Faradisation will on the other hand stimulate in a similar way to counter-irritation and passive motion, and will only increase functional activity by acting indirectly on organic nutrition. Galvanism acts upon the functional activity through organic nutrition, whilst faradism acts upon organic nutrition by the increase of functional activity.

In applying these forms of electricity to the different parts of the body, we find that the galvanic current has a powerful effect upon the brain, producing vertigo when applied to a single hemisphere, probably by disturbing the equal action of the two halves in the sense of equilibrium. The nerves of sight are influenced by different currents, and more especially by such a direction of the current as to bring the optic lobes, the optic nerve, or the retina, within its scope. Taste is affected by a current through the occiput; the medulla oblongata, or the region of the mastoid process.

The faradic current produces none of these results, and on this account it has been denied that it could penetrate the cranium. The absence of phenomena is probably owing to the want of muscles of motion and nerves of sensation. As might be expected, aches and uneasiness in the head are much oftener relieved by the galvanic than by the faradic current. In chronic ophthalmia, after the acute symptoms have subsided, a feeble galvanic current has a marked effect in stimulating the circulation, clearing up the vision, and hastening the repair of the organ. The faradic current is less marked in this direction. In the chest, a neuralgia or the remnant of an old inflammation is best treated by the galvanic current, while a paralysis of the intercostal muscles should receive most benefit from the faradic current. In the abdomen, torpor of the liver, enlargement of the spleen, and deficiencies of innervation, dependent upon the condition of the sympathetic ganglionic centres, indicate the use of the galvanic current, while in muscular torpor the faradic is the form which will best relieve the symptoms. (*St. Louis Medical and Surgical Journal*, June, 1875, vol. xii.)

Treatment of Nasal Catarrh.—Dr. William Porter, after giving a brief description of the anatomical disposition of the parts implicated in nasal catarrh, the forms, symptoms, and etiology of the disease, proceeds to consider the treatment that should be adopted, and states that there are four points to be considered. The *first* has reference to the predisposing cause, the constitutional cause which must be rectified. In the asthenic type, iodide of iron, or iodoform and iron, with cod-liver oil are generally indicated. The treatment of catarrh from syphilis is obvious. If there be ulceration, iodide of potassium with ammonia, and some form of tonic are called for, but if no ulcers exist, the bichloride of mercury in small doses, if persevered with, has no equal. These cases are much more manageable than those of scrofulous origin.

In the catarrhal diathesis which we have noted, the majority of cases seem to do well with some preparation of phosphorus. This, with the addition of nux vomica, has been useful in relieving obstinate constipation and nervous lassitude in patients of this kind.

In all these conditions, good food, rest, and regular habits of life must be enjoined. The social enjoyment and happiness of the patient should not be neglected, but if possible improved, knowing as physicians must how great is the influence of the mind upon the nervous system, and of the nervous upon the nutrient.

Secondly.—The local cause of the catarrh must be removed. If there is a polypus it should be inclosed in a snare, or seized with the forceps, and be brought out. In the glandular hypertrophy mentioned, the part may be touched with nitrate of silver, by aid of the rhinoscope, and the more pendulous portions destroyed, whilst all that would irritate the nasal mucous membrane must be avoided or guarded against. But this alone will not suffice. The membrane is already hypertrophied, and the secretion abnormal.

A *third* important item is to keep the part thoroughly cleansed, so as to remove all the adherent mucus and incrustations. Nothing does this better than a weak solution of common salt in tepid water. It is not enough that it should be used through the nasal douche. He thinks that the nasal douche should only be used by the physician himself, and that the solution should never exceed a drachm of salt to the pint of water; that the salt should be well dissolved, and that the pressure should not exceed that of a column of water of twelve inches. These requirements, if carefully attended to, will obviate any danger. That so few cases have been recorded in which injury from the use of the douche has occurred, amongst the thousands who daily abuse it, sufficiently attests its harmlessness if properly applied. This means, while valuable, does not reach the upper and sometimes the most important part of the cavity. An easy way of effecting this is to attach to the douche a tube with an aperture on one side, which may be passed into the nostril, through which a stream of the solution may be directed upward toward the part, or the tube may be attached to a nasal syringe. Dr. Rumbold recommends a catheter with numerous small apertures, through which a spray of air and water is sent with good effect. If the disease has become ulcerative, a deodorising solution of permanganate of potassium or salicylic acid may be used in a spray, after the cleansing.

Fourthly and last, but most important, is the local medication. If there are ulcers they may be touched with iodine in glycerine and water, with the addition of a little iodide of potassium, or with weak solutions of nitrate of silver. The latter is most useful where there is thickening of the membrane. Hydrate of chloral directly applied answers a good purpose where the ulceration is sluggish. It may be used from five to fifteen grains to the ounce of the menstruum. Where the thickening is not marked iodine vapour does well. A few drops of a concentrated solution of iodine containing a little conium or lupulin may be placed in a Roosa's inhaler, and the vapour propelled by means of the double bulbs or a small bellows. This should be used for at least fifteen minutes each day. After a few applications of this vapour a free flow of serum is induced, which lessens the infiltration and its attendant symptoms. If there be deafness from narrowing of the Eustachian tube, it may be sometimes much relieved if the vapour is passed along it by means of inflation by the Valsalvian method. In many cases the frequent use of a snuff composed of camphor, tannic, and salicylic acid is advantageous. (*St. Louis Medical and Surgical Journal*, November 1875.)

Treatment of Ozœna.—Dr. A. d'Azambuja first draws attention to the fact that the term ozœna only indicates a symptom, namely the fœtid odour of the air expired through the nose, a symptom that may occur in various forms of coryza, and which he has satisfied himself may be observed in chronic ozœna without any ulceration, In most instances ozœna results from some constitutional affection, as from scrofula or syphilis, and as a sequela of various forms of infectious diseases; occasionally it is a consequence of eczema of the mucous membrane, injuries, &c., in persons who are otherwise in sound health. In regard to its treatment he chiefly recommends the use of the nasal douche and the direct cauterization of the ulcer, if any ulcer be present. Necrosed portions of bone must be removed. It is only in very extreme cases that he can admit the applicability of Rouge's severe operative proceeding (reflection of the whole upper lip after division of the septum nasi through the mucous membrane of the nose), Rouge having lost one case in nine operations from meningitis. (*Centralblatt f. d. Chirurgie*, No 38, 1875.)

Action of Croton-chloral-hydrate.—Dr. N. Jerusalimsky has made a series of experiments with Croton-chloral-hydrate, the results of which show that this agent possesses the power of diminishing the pressure of the blood, and that the diminution occurs both with the cerebral centres uninjured, and after their action has been abolished; it therefore exerts a paralysing action on the heart. When small doses were administered the respiratory activity was retarded; with larger the respiration was arrested. Much larger doses are required to produce sleep than in the case of chloral; control experiments showed that it possesses no specific influence on the fifth or any other sensory nerve. No sleep was obtained by the administration of forty grains in a case of prosopalgia, though it produced malaise, tightness of the chest with feeling of exhaustion, and a pulse of 120. On making the patient breathe oxygen the number of pulsations fell considerably. In a second case in which it was given a neuralgic affection from which the patient suffered was cured, and in a third the pain was alleviated. (*Mosk. Med. Zeitung*, 1875. No. 17.)

Notes and Queries.

NOTES AND CORRESPONDENCE.

WHISKY.—Messrs. Bernard of Leith have sent us a specimen of their D D Encore Whisky, said to contain no fusel oil. There can be little doubt that fusel oil is much more injurious to the nervous system than ethylic alcohol, and that spirits containing any large proportion of it are much more likely to cause headache and other disagreeable symptoms than those which are free from it. It is therefore desirable in many instances where the use of alcohol is indicated to employ a spirit free from admixture. In order to be certain of the quality of the whisky sent us we have had it analysed by a most accomplished chemist, and he informs us that whereas many specimens of whisky contain so much fusel oil as to become muddy on admixture with water, Messrs. Bernard's contains none at all. It may therefore be safely recommended in cases where spirits are desirable as a stimulant. Its excessive mildness may also be an advantage in the eyes of those who are unaccustomed to whisky as a beverage, though it may be considered a drawback by confirmed whisky drinkers.

ON THE TREATMENT OF SPASMODIC ASTHMA BY THE SUBCUTANEOUS INJECTION OF MORPHIA.—Dr. John Kent Spender writes:—"Dr. J. Keith-Anderson justly says of this plan that it is so simple and so obvious, that he thinks it must have occurred to others besides himself, though he can find no literary mention of it. He need not have travelled further than the last edition of Dr. Tanner's "Practice of Medicine," edited by Dr. Broadbent, and he will find the practice favourably spoken of (vol. i., p. 599). I have constantly resorted to hypodermic medication with morphia, and in scarcely any case have I been disappointed during the last six or seven years. And perhaps you will allow me to quote the following passage from my Fothergillian Essay on 'Therapeutic Means for the Relief of Pain' (p. 193): 'Whenever asthma defies the common ways of common physic, I submit that we ought never to refuse a patient the relief which is ordinarily brought by putting morphia under the skin.' I go further than this now, and often begin my treatment with hypodermic morphia; and certainly never delay it longer than to ascertain first whether the malady is controllable by a combination of belladonna, chloral, and iodide of potassium. It is remarkable that in the monograph on asthma in Dr. Reynolds's 'System of Medicine,' by the late Dr. Hyde Salter, there is not a single allusion to opium or morphia in any form or by any method of administration."

BEEF TEA.—The Concentrated Beef Tea, Essence of Beef, and other preparations of Messrs. Brand, although much esteemed by many members of the medical profession deserve to be still more widely known. Their Concentrated Beef Tea, which looks like a small sausage, is most convenient for transport, and a little of it dissolved in boiling water makes a most agreeable and well-flavoured beef tea. This preparation of Messrs. Brand proved a most useful stimulus to the troops on the Ashanti Campaign, under circumstances where spirits signally failed to be of service.[1] The Essence of Beef consists solely of the juice of meat extracted without the addition of water, and forms a thin jelly which has a most agreeable flavour, and can be taken without the addition of water. Both of these preparations we consider to be most valuable aids in keeping up the strength of patients during acute disease, or restoring it during convalescence. A recent visit to Italy and the South of France has also convinced us of the desirability of patients about to spend the winter in any of the smaller health-resorts there, providing themselves with Concentrated Beef Tea or some similar preparation; as soups, though abundant, are by no means always of the most nourishing description, and a basin of beef tea made in the patient's own room with the aid of an Etna might not only prove very comforting and reviving after a walk, but counteract the effects of any slight over-fatigue.

[1] Vide *Practitioner*, August, 1875, p. 120.

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⁂Any of the foreign works may be procured on application to Messrs. DULAU, of Soho Square, W.C.;—WILLIAMS and NORGATE, of Henrietta Street, Coven Garden, W.C.;—or BAILLIÈRE, of King William Street, Charing Cross.

Department of Public Health.

THE HEIDELBERG PAIL-SYSTEM OF FILTH DISPOSAL.[1]

[IN 1870 a Commission was appointed in Heidelberg to report on the cleansing and draining of the city. The report prepared by Dr. Mittermaier contained recommendations for a supply of pure water, the construction of drains for the removal of slop-water, and the disposal of human filth by a pail-system. Since the publication of this report the city has obtained an excellent supply of good water, a plan of drainage has been designed and is now being carried out, and a pail-system of filth-disposal has been introduced to some extent, this latter measure, however, being the voluntary work of some individuals, not the work of the municipality. The following is the account which Dr. Mittermaier gives of the pail-system adopted in Heidelberg.]

As its name indicates, the pail-system consists essentially in this, that in each house and in the lower part thereof, small movable and portable pails are placed, into which all the privies situated in the parts of the house above open by a common soil-pipe. These receptacles are daily or every few days removed, and other empty ones placed in their stead. The full pails are removed to a special place outside the city, when the contents, either directly or after undergoing some preparation to fit them for that end, are used as manure.

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[1] From the 4th Section of *Die offentliche Gesundheitspflege in Städten und Dörfern*, by Dr. Mittermaier, of Heidelberg (Carlsruhe, 1875, p. 35). Dr. Mittermaier visited England in the course of 1875 to study for the second (if not the third) time, English sanitary work in towns and villages. During his visit he promised, *apropos* of his examination of pail-systems of filth disposal in certain of our northern towns, to favour the *Practitioner* with an account of the pail-system lately introduced into Heidelberg. He now sends the above-named pamphlet in which he includes the account he had proposed to draw up for this Journal.

This system is carried out in different manners in different places. The receptacles, of various shapes, are made of wood, or tinned or painted sheet-iron; the soil-pipe is also made of various materials, wood, iron or clay.

In some places the soil-pipe is made to open freely into the pail, in others it is most carefully fitted to the pail, a siphon water-trap intervening to prevent the gases from the pail entering into the excrement-pipe. Ventilation is also cared for in different ways. In general the soil-pipe is prolonged upward to the roof, or a special ventilating pipe, branching off from the lower part of the soil-pipe, is carried up by the side of the chimney to the open air above. Further, in some places, in the privy pan itself, there has been an arrangement made for separating the solid from the liquid excrement, with the view of keeping them apart and, by the addition of some matter, preventing decomposition. In other places, as in Zurich, for example, a separation is made in the pail itself, the liquid contents flowing through a screen bored with holes, into a pipe which is connected with the sewers, the object being to lessen the amount of material to be removed, as it was thought that the liquid could not be used with advantage upon the land. For the most part, however, the solid and fluid excrement are received undivided into the pail and carried away together. In some English towns, as in Rochdale and Birmingham, a special disinfecting fluid is placed in the pails to prevent decomposition and the bad smell arising from the contents.

The pails in Heidelberg are made of strong sheet-iron, and are firmly connected with the soil-pipe, a siphon-trap being interposed at the point of junction. The pail is of a cylindrical shape, 32 inches high and 18½ in diameter, holding about 23 gallons. It is provided with handles, so that it can be easily removed and carried by two men. It is provided with a cast-iron ring, by means of which the extremity of the soil-pipe when put in its place can, by a turn of the hand, be firmly connected and fixed. After the connection a handful of ashes may be placed over the spot to prevent the escape of gas, &c. At the side of the pail and upper part is a small pipe, 15 millimètres (rather more than half-an-inch) in diameter, through which, when by any neglect (as, for example, when too much water has been thrown into the privy) the excess of fluid can escape. To receive this a small vessel is provided. This pipe may be filled with charcoal, by means of which the escape of foul gas is prevented. This overflow is very rare, and only occurs in cases of great carelessness. With the exercise of moderate care an overflow rarely happens.

The fact that the above arrangements have held good for the last six years speaks strongly in their favour.

In order to prevent any misunderstanding we must repeat that the pails are not to be placed on the different floors, but only in the lower part of the house; either in a niche specially built for the purpose, or free in the passage, or in the site of the old privy, or in the cellar. Two pails placed side by side and connected together above by a waste pipe, have been employed in some densely populated houses where the employment of the ordinary pail was impracticable. In some thickly populated buildings, also, such as hospitals, schools, or large private houses, larger transportable wooden pails are placed, which are connected with the soil-pipe in the same manner as the iron ones. The iron soil-pipe is for the purpose of ventilation, and as directed by a ministerial order of last year, carried upwards of the same size, and as nearly as possible perpendicularly out over the roof of the building. The seat of the privy is arranged in various manners according to the desire and pretensions of the possessor. If the ordinary water-closet system is adopted, water should only be sparingly used.

The management of the pail-system is carried out by a committee appointed for the purpose by the possessors of the pails. The committee appoint an overseer, and the necessary number of workmen and waggons. The time for the removal of each pail is settled, and in this respect no complaint has been made of any nuisance or irregularity arising.

The changing of the pails—viz. the taking away of the full pail and its replacement by a clean empty one—occupies but a very short time. A closely-fitting cover is screwed on, and the full pails placed on a low waggon are taken to the emptying place outside the city. Here they are raised by a simple contrivance and immediately emptied into large casks (from the country) containing about 2,000 litres (440 gallons), and then they are cleaned out. The contents of the casks are, without delay, employed for manure. By an arrangement with the husbandman this is all exactly managed.

The present committee also provides for the removal of dry rubbish. The ashes and contents of the dust-bin are made use of for filling up excavations; the kitchen waste and sweepings for mixing with the collected excrement. With the latter the water used in cleansing the pails is also mixed.

In this way by means of a voluntary association the management of the pail-system has been carried out. We may add that the property of each possessor is marked with the *street* and *number* of the house to which it belongs. Of course the whole management would be much simpler if it were undertaken by the municipality and all the plant belonged to it and not to different individuals. But even as carried out at present in Heidelberg we are convinced that the system has proved a success.

Much has been said about the unpractical nature of the pail-system, but every one who has seen the system in actual working has always expressed complete satisfaction therewith, so far as our experience goes.

Indeed the advantages of the system must be evident to every unprejudiced observer. In houses supplied with pails, which were formerly subject to the most unpleasant smells issuing from the privy-pit or the drains, the inhabitants at present enjoy pure air, and the assurance that their health will no longer be injured by these hurtful emanations which were formerly given off from the moist and saturated brickwork of the privy-pit.

Hence is explained the circumstance that a row of houses which before did not enjoy favourable health conditions, since the adoption of the pail-system, has been spared any attacks of disease. So it comes that many houses which formerly almost yearly were attacked by typhoid have since been free therefrom. By the aid of the pail-system we have been enabled to remove many sanitary imperfections of the worst kind where the old drains ran *under the houses,* and were in all probability the only cause of local epidemics of typhoid. In the war years 1870 and '71, the pail-system was particularly successful, and prevented (in the numerous reserve hospitals—mostly lazaret barracks—where more than 2,000 wounded, either with typhoid diarrhœa or dysentery were cared for) the seeds of disease being spread elsewhere.

This was especially the case in the so-named epidemic barracks, where those soldiers suffering badly from typhoid or dysentery were placed. It would take us too long in this place by the detail of special cases to prove the correctness of the above statement.

As the inconveniences of the pail-system have been much insisted on by its opponents, we may here emphatically remark that their fears have proved quite groundless. The changing of the pails, especially here in Heidelberg, is accomplished without any disturbance of the household, and can be made at any time of the day.

As we have remarked, the different parts of the present pail arrangement have here been practically tested. This is especially the case with the arrangement by which the connection of the soil-pipe and the pail is established. The siphon-trap described has been found to act most satisfactorily, as the rinsing-out water, which must at least be poured down the privy-pan, once daily, in cleaning out the night vessels, serves to cleanse and keep the trap charged. As the diameter of this trap, like that of the soil-pipe, is only 8¼ inches (21 cent.), the diffusion of gases, the products of decomposition, through the trap is very inconsiderable, and is in every case by the present ventilation of the pipe rendered harmless. The plugging of the siphon-trap—a danger so much dreaded by many—with the exercise of a very moderate amount of care hardly ever takes place, and even when it does is very easily remedied.

As a matter of course in this system, as in every other of modern use, it is absolutely necessary that the custom of throwing *everything* that one wants to get rid of down the privy, must be broken. But whereas in the pail-system any stopping up can be removed with little trouble, it often happens in the water-system that to expose the obstruction it becomes necessary oftentimes to open out the drain quite down into the sewer to find the source of the difficulty. No difficulty whatever occurs in the ventilation of the pail and privy if constructed as above directed, attention being especially paid to the length and perpendicular direction of the air-pipe: it is sufficient both in warm and cold weather, so that even the closing of the privy pan is not necessary.

Recently in Berne Dr. Vogt has proposed a direct ventilation of the pails, and found the use of the siphon-trap unnecessary. For that purpose, as already hinted, directly over the pail a branch pipe is connected with the soil-pipe and carried up by the side of the chimney out over the roof. By the warmth of the chimney a continual current of air upwards is produced in the pipe, which sucks up the injurious gases from the pail, so that they escape into the upper air. Should this contrivance prove successful the lengthening of the excrement-pipe, or perhaps even the use of the siphon-trap may be needless.

As regards the shape of the pails it may be remarked that the long, narrow form and small size of the same render them appropriate for erection in a limited space, and easy of removal along a narrow passage.

The preference is given to iron as the material of the pails as being impermeable and not liable to get soaked like wood. When they are made of sufficiently strong sheet-iron, the riveting good, and fresh painted once a year, within and without—their durability leaves nothing to be desired. Pails which have been in use here for six years require only a little mending, and at the most a new bottom. No pail has become useless. It is evident that the pails should not stand in the *wet;* also that the pouring-in of highly concentrated disinfecting liquids tends to destroy the vessels sooner than otherwise would be the case. Wooden pails are cheaper than iron ones; and where circumstances do not permit the procuring of iron pails, the use of wooden ones should not be rejected.

As regards the changing of the pails, according to our experience, when 15 persons use the pail, this should be done three times every week. If the number of persons be less, then twice weekly may suffice. If one reckons on an average the excrement of each person at 3 pounds = 1½ litres daily, then the average produce of 15 persons (the average number in a household) will be 45 pounds = 22½ litres in two days, 45 litres in

3 = 67½. When therefore the pail has been used for 3 days, there will yet remain for rinsing-water and any extra excretion a space of 30 litres; so that even then the pail will never be quite filled.

It is highly important that close to the privy or in it, and on every floor of the house, there should be a sink in connection with the street drains (sewers). Into these can then be thrown in the most ready manner, the slops and dirty water, while the contents of the night-vessels and the accompanying rinsing-water must be emptied into the *privy*. By this very simple contrivance one great source of objection often made against the pail-system can be removed. Many, for example, hold that it is impracticable to keep the slops from the privy and the urine from the sewers. We can assure these unbelievers that we have found no difficulty in carrying out this regulation, and we maintain the separation of the different fluids.

Certain fears which we entertained about the freezing of the contents of the pails have proved unfounded. In no single case has this happened, although the winters have been severe. In the very severe cold of last winter two of the *siphon-traps* were frozen; but one stood out in the open air, and the other was insufficiently protected. The same kind of thing happened in this case as might occur with the water-pipe or the siphon-trap of the English water-closet; still with this advantage on the part of the pail-system that the wide siphon-trap of the soil-pipe can be easily opened and the evil removed.

As before remarked, the adoption of the pail-system in this city is entirely optional, and depending, as it does, upon the will of individual persons has developed itself but slowly. Notwithstanding this, it has been adopted in nearly all the *new* rows of buildings, and also in a number of private and public buildings formerly supplied with cesspools or sewers. It has become evident that even in very narrow places, where sometimes no small difficulty had to be conquered, the system was applicable. Every care was taken that the removal of the pails should be as easy as possible. In general it was arranged that the pail could be readily carried from its standing-place and placed in the waggon. In a very few cases was it necessary to bring a small machine to raise the pail.

The number of buildings in which the pail-system is at present applied is over one hundred, and the number is continually increasing. Among them are to be found the small modern houses of the working classes, and the houses of the richer classes provided with every luxury,, many school-houses, and the greater number of the hospitals.

The repute of this system is not confined to this city and its neighbourhood, but it has been adopted in 41 other places; and in these also its use is not confined to private houses, but has extended to schools, hospitals, and noblemen's seats. From every side we receive most favourable reports and expressions of satisfaction, and the application for further supply from the makers here continues to increase. Nürnberg has recently rendered the adoption of the pail-system according to our pattern compulsory for all new buildings, with a view of extending it gradually throughout the city.

It now remains to consider the financial side of the question. The cost of the system in the present houses here is as follows: for the two pails, siphon-trap, and all belongings, and including the cost of erection 215 marks (£10 15*s.*). In those cases where the privies were in good condition, no further outlay was required. Where a special place has to be erected for the pail, of course the outlay is in accordance therewith.

Thus it will be seen that the cost of construction, even in accordance with the strictest requirements of the pail-system, never reaches that of the cesspool, constructed with the latest technical improvement, and is considerably less than that of a water-closet and the drains leading to the sewer in the street.

For the changing, fetching away, cleaning and bringing back of the pails, the company receives from the owner 20 pfenings (= 1 penny sterling)—reckoned by the year about 2,000 marks (£100). For the contents of the pails sold the yearly income is rather more than 1,700 marks (£85); as for the 150 litres — 16 kr. = 46 pf. is paid. For the removal of "refuse and dust" from the houses the income is over 1,500 marks (£75). For compost made out of this and horse-dung about 300 marks (£15); so that altogether *the year's income is* 5,500 marks (£275).

The expenses in the current year were 5,530 marks (£276 10*s.*); income, 5,500 marks (£275); so that the cost of working is covered by the income; twenty pfenings (= 1 penny) being received for the removal of each pail. Inasmuch as the use of the pail-system is on the increase, the expenses will increase less in proportion than the income, as the greater the number of houses using it, the less expensive in proportion will the removal of the pails become. Then, instead of going from one end of the town to the other to fetch the pails, the same vehicles can collect them in the streets of one quarter of the town only and so economise time and labour. It also seems probable that with increased competition the price of the *pail*-contents will also increase. This is to be expected according to the experience of men well versed in the matter.

The association since its establishment has not failed to have recourse to the counsel of experienced agriculturists, and of men such as Prof. Fühling, Dr. Hertt, Prof. Mayer, and Prof. Stengel, who combine scientific knowledge with practical experience. These have unanimously reckoned the agricultural worth of the contents of the Heidelberg pail for the land (that is, after deducting the cost of transport from the emptying spot to the ground) as at least 24 kr. = 69 pf. (about sevenpence) for 150 litres. The excellent condition of the ground manured by the contents of the Heidelberg pails (situated about a mile from the town) is a proof of the good effects of this kind of manure.

In the above account we have endeavoured, in strict accordance with the truth, to describe the circumstances as they have presented themselves in Heidelberg. One sees therefrom that the untiring exertions of a voluntary association have been able to conquer numerous difficulties and to effect much good. There is no reason why the same plan should not be tried with equal success in other places. It is obvious that the undertaking would be everywhere rendered much easier if this important measure from the beginning were taken in hand by the community.

Something more now remains to be said as to the application of the pail-system to country districts, although this question has already been touched on above.

The necessity for a complete change of the existing system we have already proved in Part II. of the present work. We have also shown that little assistance can be derived in the country by the adoption of the water system or that of Liernur. Many acknowledge that the pail-system would be the best to adopt in the country districts; but are at the same time of the opinion that it would be impossible in such districts to carry it out. In this sense Herr E. A. von Göler has lately written in No. 18 *Des Wochenblatts des Bad. landur. Verein.* He is of opinion that to work the pail-system it is necessary to have a manager, and that in many country places it would be impossible to find one. From what we have advanced on this matter, the author will doubtless have been convinced that even in the smallest country towns and villages there is no reason why the pail-system should not be introduced by the formation of a voluntary association as it has been in Heidelberg. When once a beginning is made the community will soon be convinced that the management of the pail-system is by no means so difficult as at first imagined. They will see that the pail-system, for the inhabitants of a place and for the well-being of the people, is attended with the greatest advantages. This system is for the public good, and therefore deserves to be taken in hand by the community with the water and gas systems. As a rule, in the country the system might be carried out with a very small or even no sacrifice of money, and even should an addition be required from the public purse, surely no expense could be more easily justified than that made for the good of the public health.

In no case is a special contractor for the excrement necessary, although circumstances in many places, as in Graz, and Augsburg, make such a contractor welcome. The most just and best plan is that the excrement should be looked after by the municipality, as it is in Zurich and in English and Dutch cities.

In small cities and villages, when the dwelling-houses are far apart from one another, and when also each house has its own garden and field, the excrement can be employed by the house-owner. He can at once use the contents of the pail upon the land, or should it not be required anywhere at the moment of its production, can preserve the same till wanted as a dung-heap. Then the interference of the municipality will only be required to insist that the proper amount of care and cleanliness should be exercised. It must, for example, specially be insisted on, that the heap into which the contents of the pail are thrown should be immediately covered with earth, ashes or dry rubbish.

As to the construction of the pail, we have already suggested that it can often be made in a much simpler manner in the country than is allowable in a large city. In the country, for example, it may even be permissible to do away with the siphon-trap, even a special arrangement for ventilating can in a free open place be dispensed with, when the pails are emptied and thoroughly cleansed every day, or, at longest, every second day. Even in the country, however, it is necessary that the essential conditions of a good *pail*-system, as described, should be strictly adhered to—viz. impermeability of the pail, impermeability of the soil-pipe, the close fitting of the latter into the pail, and complete covering of the pail during its transport. Should the privy of the house be on the *ground* floor without soil-pipe, and the privy be completely separated from the house, then the privy-pan may open directly into the subjacent pail; but under these circumstances there must be a ventilation shaft above for the tun and the privy itself.

According to our conviction the following conclusions seem to be justified from a careful consideration of all the circumstances detailed above:—

1. A well-managed pail-system, as that of Heidelberg, satisfies every sanitary and technical requirement. After a perfectly unprejudiced comparison, it may be declared as equal to the best systems, and it possesses indisputably certain advantages peculiar to itself.

2. Such a system can be easily introduced and carried out under the greatest variety of circumstances both in cities and villages. The cost of working it can under proper management be covered by its receipts: perhaps even under favourable local circumstances the undertaking can be worked at a profit.

3. This system also at the same time satisfies the well-deserved claims of agriculture.

4. The system interferes less than any other with the introduction of any better plan which may hereafter be discovered for the removal of excrements. It can easily be replaced by any other plan (if needful), and the charge of conversion will require the least possible sacrifice of money.

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ON THE SELF-ADMINISTRATION OF CHLOROFORM.

BY JOHN M. CROMBIE, M.A., M.D.

IT is well worth considering why many persons, by no means indifferent to life or its obligations, risk themselves in the self-administration of chloroform, well knowing at the same time the danger they may be thus incurring. The fact acquires additional importance, inasmuch as such procedure is adopted as a rule not in compliance with, but strictly in opposition to medical advice. Let it be granted that nobody need be afraid at the outset to pour some few drops of chloroform on a handkerchief, even from a full bottle, with the view of obtaining sleep, when they take care in the first place that the bottle is removed to a safe distance, with the stopper carefully replaced. But when the inhalation is once commenced, to keep the bottle at hand, and then, in a state of drowsiness, when the sense of relation is lost, or confused, to add therefrom a few more drops a second or third time, and so on, is a proceeding which no one possessed of any intelligence and knowing anything of the properties of chloroform can fail to see must be fraught with the utmost danger. Yet many persons, with no lack of intelligence, fully conscious of the danger, and against the wishes of friends and advisers, deliberately do this.

It is obvious enough the motive must be a powerful one to overcome a reluctance so strengthened on every side by disapprobation and danger, and cannot indeed rationally be interpreted to mean anything other than this,—that incapacity for sleep, through pain or watchfulness, may amount to utter and intolerable wretchedness. In no other way can a departure so decided and direct from the guiding instincts and inclinations of human nature be fairly accounted for, and if it be rightly considered, such conduct is itself the most striking exponent of the state from which it springs. From neglecting or ignoring this view of the case many are content to join in the condemnation of the practice as wholly unjustifiable and unnecessary, which, of course, is a very easy thing to do, and may be made an occasion for the display of much caution and prudence; but of what value or weight is a professional interdict when the patient leaves the doctor behind because he cannot rise to the situation, and draws back denying its very existence?

It is not intended for a moment to be put forth that those only have recourse to the self-administration of chloroform who have proved its utility as the only efficient agent in the last extremity of distress; but I wish to draw attention to their case by themselves, so as to bring out distinctly their rightful relation to the methods of scientific medicine. And this is a remark which deserves attention, because it might justly create surprise if so grave a remedy were to be recommended for simple sleeplessness. But let it be understood that we are now speaking only of those who, by a process of trial and failure of what other ways and means there are, have at last been driven to adopt chloroform frequently at their own risk, and often, as has been said, in direct opposition to medical advice. The reason for gravitation in this direction lies in the nature of things, and is not very hard to discover.

With chloroform sleep can be counted on as a certainty; with none of the other narcotics, such as morphia and chloral, all equally dangerous according to present modes of administration, is such the case. Sometimes, from too small a dose of these, additional restlessness is produced; sometimes only stupefaction, not sleep; sometimes, from too large a dose, the sleep may extend over many hours, and the waking state itself remain a condition only of pitiable somnolence. Much human misery may be compressed into a single sentence, and the last is an example, as those who happen to be the subjects of such miscarriages soon learn to know. The difficulty with such drugs, administered in the ordinary way, is that the dose cannot be graduated, at once, or by successive additions so that just enough and no more shall be taken, simply because some time must elapse before it is known how far the quantity taken has developed the tendency to sleep. With the inhalent narcotics, on the other hand, such as chloroform, the tendency to sleep is developed *pari passu* with the inhalation, and admits of being checked as soon as the desired result is obtained. The potent energy of the drug, therefore, let it be observed, though it may justly be the cause of apprehension when improperly handled, is the very quality in it which makes possible an accurate adjustment to the requirements of the case.

Then an exceedingly small quantity of chloroform is found sufficient to allay restlessness and dispose towards sleep, which in cases of extreme nervousness, creating watchfulness and anxiety, is, so to speak, waiting to be wooed. It needs only to be provoked by the most gentle encouragement. Indeed, I have often thought the steadiness of mind created by the confidence reposed in the agent was more than half the cause of its success. Now the result of this is that the sleep so induced is natural; the little chloroform that has been inhaled, unlike morphia or chloral, passing off rapidly—a consummation that is devoutly wished by all who have to employ artificial means to obtain a night's rest. To awaken without "a drag and a weight and a dizziness in the eyes at morn" is hardly less a boon than that of sinking without effort and without delay into slumber. But although such is the experience of many, it is not every one nor on every occasion that the result is so easily obtained. It ought to be particularly remarked that those who have been much accustomed to stimulants are the least likely to obtain the benefits of chloroform on those terms. It is most unquestionably with the temperate that the agent acts with its greatest charm, and this is perhaps one of the reasons why ladies are so often the persons who have recourse to it, and why also, unfortunately from quite separable and incidental circumstances, they have, nevertheless, been oftenest among its victims. The second point referred to is of no less importance—the conditions, that is to say, that render a minimum of the agent effective. Among these are such as are familiar to all who have in the least observed the action of narcotics, particularly a more or less empty condition of stomach,—rest, quiet, shade; but above all, the condition that the heat of the body is perfectly normal. Compared with all other circumstances, in disposing to sleep or in helping the action of chloroform, this is simply colossal. Proper warmth must be provided as an indispensable preliminary. If the feet and hands are cold, or any tendency whatever to chilliness exists, the patient by an overdose may be numbed into unconsciousness, but it is like the winter sleep of the animals in the arctic regions—obtained by next to extinguishing the functions of life. When, on the other hand, the circulation has been got into good trim, and a feeling of warmth prevades the system, a few drops of chloroform, like the after-dinner glass of wine, go to produce that grateful sense of fulness and calm which becomes the immediate precursor of sleep. Although this all-important rule as to temperature is often lost sight of in the administration of chloroform, to the imminent risk of the patient, it must strike even the uninitiated, one would think, that there is a vast difference between those two modes of inducing insensibility.

From this it will be seen that it is because of its special and unique fitness for overcoming with the least inconvenience the most tormenting distress of disease or exhaustion—sleeplessness, that chloroform has forced itself forward as a remedy, with little sanction from the medical profession, indeed the reverse. Yet, apart from wilful prejudice or unreasonable timidity, there are no grounds for hostility to an agent in medicine so beneficent in surgery, unless the danger of its use by self-administration cannot be overcome—the only way in which it can ever be of much general service in medicine. More than three years ago I endeavoured to show that the possibility of an overdose, the only danger in question, could easily be overcome, and since then I am happy to say that the simple apparatus recommended for this purpose has met with considerable approval by the profession, and has been useful in cases which otherwise would certainly have been exposed at any time to fatal casualties. I cannot mention this in the *Practitioner* without recording the kindness with which the late Dr. Anstie listened to my explanations on the subject at a time when, as far as I could find, nobody else would.

Those who are interested in the subject will find in the *Practitioner* for April, 1873, a woodcut and an account of the apparatus referred to. I need only say in conclusion, that if the self-administration of chloroform comes in at an emergency in a way that no other remedy will, it must be recognised, and the professional man, instead of vainly assuming the attitude of an obstructionist, ought rather to see that the best means of securing safety are at least recommended to be carried out.

NELATON'S INVERSION METHOD IN A CHLOROFORM ACCIDENT.

BY LAWSON TAIT, F.R.C.S.

On July 7th, whilst performing Amussat's operation on a thin, delicate, and much exhausted patient, my attention was suddenly drawn to the fact that the respiration and carotid pulsation had ceased. I tried to feel the beat of the heart, but, failing to do so I immediately seized the patient round the waist, inverted her and kept her with her thighs bent over my arm and her head hanging downwards. I then directed my colleague in the case to imitate respiration by compressing the chest at intervals of about five seconds. Looking back now at the case it is probable that pulsations began to be felt at the root of the neck in about two minutes after she was inverted, but it seemed to me like a quarter of an hour; and it must certainly have been fully five minutes before she resumed respiration independent of assistance. I kept her in the inverted position for about five minutes longer.

Any one who has seen a death from the uncomplicated action of an anæsthetic must know how useless are the directions for galvanic batteries. Such appliances are sure to be out of order when most needed, and the benefit to be derived from them even when they act is very problematical.

The cases narrated by Dr. Marion Sims were enough to convince me that the inversion method, whatever may be the theory by which its success is to be explained, had rescued patients from impending death; and I am convinced that it saved me from the loss of the patient whose case I have narrated.

Since July I have used nothing but anhydrous sulphuric ether for operations, and though it is far from being so convenient as chloroform or methylene ether, I think we are not justified in using any other anæsthetic, save in the cases of young infants and pregnant women, amongst whom no chloroform accident has yet been recorded.

THE CAUSES AND TREATMENT OF SLEEPLESSNESS.

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SLEEPLESSNESS is one of the most troublesome ailments which medical practitioners are called upon to treat. It is annoying to the patient—to how great an extent, probably, only those who suffer from it can appreciate; it is troublesome to the practitioner from its frequently intractable character. Much, however, of the difficulty of treating insomnia satisfactorily lies in the complex associations of the malady. The measures which will readily procure sleep under one set of circumstances fail signally when employed under other circumstances; and yet the causes of such failure often remain undiscovered because they are not studiously sought for. In order to render the subject more intelligible, it may be well to glance at the physiology of sleep, and then to proceed to review the different forms of insomnia, with the form of treatment appropriate to each.

Sleep is a condition of physiological cerebral anæmia. At one time—not so very long ago—it was thought that sleep was connected with a state of vascular congestion of the brain; this view took its origin in the vascularity found after fatal doses of opium. In reality, in such fatal cases there is marked venous congestion of the brain; but then the condition preceding death here is fatal coma, not normal sleep. The experiments of Durham, Hammond, Donders, and others, have placed beyond doubt the relations of sleep to cerebral anæmia. Such conclusion might have been anticipated from the physiological law—that the functional activity of an organ is in direct proportion to its blood supply. This condition of anæmia is produced by the co-operation of two factors: one, a modification of the vascular system; the other, a diminution of activity in the cerebral cells themselves. It is the combination of these two factors in the production of sleep that must ever be borne in mind in the treatment of each case of insomnia. According to the predominance of one or other of these factors must the remedial measures be selected and proportioned, if they are to be successful. The measures which would be most appropriate to the relief of insomnia in the convalescence after a fever, would be most unsuitable in the sleeplessness of the gout.

It will, perhaps, be well to take the simpler forms of insomnia first, and then to proceed to the more complex and intractable forms. With many persons sleep is unattainable if their feet be cold. This condition, depending upon contraction of the arterioles of the lower limbs, is not confined to the extremities of the limbs, though, of course, it is most pronounced there. It affects a large portion of the vessels of the body, and by thus, as it were, driving the blood to the head, keeps up a condition of arterial vascularity in the brain, which effectually prevents sleep. Here the warming of the feet is often sufficient to permit of sleep without any resort to hypnotics. In order to warm the feet, many resort to hot bottles; but a much more effectual measure, especially with young persons, is to immerse their feet in cold water for a few moments, and then to rub them well with a rough towel until they become warm. If this be done ere getting into bed, that coldness of the feet which is so inimical to sleep, will, usually, be no longer felt.

Cold is often a cause of insomnia. If insufficiently clad, so that the cutaneous vessels are not relaxed, sleep is rarely sound, when attained at all. It is only when stupefaction from cold occurs that the dangerous coma-sleep comes on, which is so often fatal. With many persons sleep is impossible if the bed be not warmed previous to their entering it. In these cases, the arteriole contraction, induced by the contact with the cold bed-clothes, does not pass readily into the opposite condition of relaxation, and so sleep does not come on. This state of affairs is very commonly met with among the aged, and most so in those who suffer from some form of heart-disease. In such cases it is not only desirable to warm the bed and the night-clothes, but even to administer some hot fluid, all the better for containing alcohol, when the patient has got into bed, if any sense of chilliness remains. In the young and healthy, the chill on getting into bed, however, is often of great service in inducing a subsequent relaxation of the cutaneous vessels, and thus conducing to more perfect cerebral anæmia and sounder sleep. Few persons will have failed to note the deep and usually refreshing sleep which follows any disturbance during the night, which has exposed them briefly to cold. Getting up to do some little service for a sick friend, or to attend to some natural call, will usually lead to a sense of chilliness, which makes the warm bed-clothes extremely comfortable on again getting into bed, and which is followed by sound sleep. Here the brief contraction of the peripheral vessels is followed by relaxation, leading to more complete cerebral anæmia.

At other times an increase of the body-temperature is the cause of disturbed sleep, sometimes amounting to sleeplessness. This is most frequently seen in febrile states, where a delirious condition takes the place of natural sleep. After the temperature has been brought down by the application of cold, a fever patient will commonly fall into refreshing sleep, until a return to a fever temperature disturbs it. In slighter conditions of restlessness, associated with too great a body temperature, the dissipation of a certain amount of superfluous heat by getting out of bed and drinking a draught of cold water, will usually be followed by sound sleep. At other times, throwing off some of the bed-clothes will achieve the same end: though, if the amount removed be too great, wakefulness may follow from a too low temperature being brought about. With some persons, when the rest is disturbed, it is sufficient to protrude an arm or a leg from under the bed-clothes, so as to secure more loss of heat; and sound sleep will follow.

At other times sleeplessness is due to a condition of high cerebral vascularity, accompanied with excitement in the circulation generally. This associated condition may be due to inflammation somewhere, which sets up the usual vascular excitement of inflammatory conditions, together with excitement in the cerebral cells, produced by the sensations of pain received by them. Or it may be due to an inflammatory condition in the brain itself; or to a condition of high cerebral vascularity falling short of inflammation, as in the paroxysms of excitement in various forms of insanity, or in acute mania. Here there will be found in varying proportions, (1) local (cerebral) hyperæmia, (2) general vascular excitement, and (3) a rise of temperature. When sleeplessness is found under these circumstances, the treatment must embrace the different factors, and include each in the remedial measures employed. Opium will not alone meet these complex states. In order to procure its hypnotic effects, it is necessary to combine it with a direct vascular depressant. The rule laid down by Graves, as abridged in Ringer's Handbook, for the administration of opium in excitement and furious delirium, was to combine it with antimony. "Each drug seems to assist the action of the other; and the relative doses must be determined by the circumstances of the case. In furious delirium, the tartar emetic must be given in full, and the opium in small quantities; while, if wakefulness is the chief symptom, the delirium being not very boisterous, the dose of tartar emetic must be reduced and the opium increased." Graves was evidently a most sagacious physician. Of course it is not supposed that these rules were solely due to him, but they evidence a most practical grasp of the difficulties encountered in practice, and the measures best adopted to overcome them. When the condition of wakefulness is little if at all associated with general vascular excitement, then the opium alone is sufficient to produce quiescence in the cerebral cells. When, however, there is much excitement in the circulation along with the insomnia, then a direct vascular depressant is needed to calm the vascular element in the insomnia. At the same time the antimony affected the body temperature. Consequently, we see the measures employed struck at each and all of the factors of the sleepless condition. In the present day we have chloral-hydrate, which combines the actions necessary for the relief of such form of insomnia. It acts upon the nervous system; it lessens the heart's action; and it lowers temperature. But if thus suitable to one form of sleeplessness, the very qualities which here render it so valuable constitute the objections to its use in other forms of insomnia.

There are some points to be noted in connection with the relations of sleeplessness to pain and to pyrexia, which are deserving of attention. Pain excites the cells of the sensorium, and such activity in them causes them to attract more blood to themselves, by which their activity is maintained. That, however, is not all. Pain has a stimulant effect upon the circulation, and causes a rise of blood-pressure in the vessels of the body generally, including those within the cranium. If pain be not too excessive, it does not induce shock; indeed, the pulse will be found to be fuller, and less compressible, while its beat is slower, during the infliction of pain. In an article by the writer in the December (1875) number of the *Practitioner*, it is written (p. 437): "The effect of the pain was to improve the pulse, which became slower and fuller under the stimulus." Consequently, then, in conditions of severe pain accompanied by vascular excitement opium should be combined with a direct depressant, as aconite, antimony, or chloral-hydrate, in order to successfully overcome the resultant insomnia. The opium will diminish the receptivity of the cerebral cells, but, alone, it is not sufficiently depressant to affect the circulation. Heat is a most powerful excitant of the heart's action, and conditions of pyrexia are ever accompanied by an elevation in the number of pulse-beats in the minute. Such stimulation of the heart causes an abnormal amount of blood to be pumped into the arteries, and this arterial fulness is hostile to sleep. The effects of a too high temperature in disturbing sleep has been alluded to before, and the effects of lowering the body temperature in inducing sleep referred to. In conditions of pyrexia without pain, opium alone is unsatisfactory as an hypnotic; it is here far inferior to chloral. Indeed, in the treatment of sleeplessness due to too high a body temperature, apyretics are distinctly called for; it may not be amiss to combine them with the ordinary soporifics, but they constitute the treatment essentially.

All powerful impressions made upon sensory nerves not only excite the sensorium, but they raise the blood-pressure. This holds good of sensations received by nerves of special sense, as well as by those of general sensation. The eyes are closed in sleep, and the cutting off of the stimulant sensations, coming in by the optic nerve, conduces powerfully to that result. Conversely, on awaking the eyes are opened, and the inrush of sensations caused thereby produces a more active state of wakefulness. If, when partially awake, the eyes are kept closed, sleep more readily returns than is the case when the eyes are opened: and similarly if the eyes are opened in the dark, there is less stimulation experienced than occurs if the eyes are opened in daylight.

Sleeplessness is a common symptom of lithiasis or suppressed gout. The patients complain that they cannot get off to sleep; it is not pain that keeps them awake,—they cannot "catch sleep," to use the expression they commonly resort to. Their thoughts travel in a circle, and the process is tiresome and barren of result, and tends to keep up the feeling of irritation experienced. When morning is far advanced sleep comes on, but it is brief, and the sufferer awakes weary and unrefreshed. Here the cause of the sleeplessness is almost entirely the high blood-pressure; though probably the effects of nitrogenized waste upon the cerebral cells are of a stimulant nature. This high blood-pressure is due to the action of the effete matter in excess upon the circulation; of which the accentuated second sound, the incompressible pulse, and the free flow of pale urine are witnesses. In sleeplessness occurring under these circumstances opium is useless, and very commonly is harmful, increasing the excitement instead of allaying it. Chloral-hydrate often is very useful, and with some gouty persons acts like a charm, though it disagrees with others. Bromide of potassium is very serviceable here, and may advantageously be combined with hyoscyamus or with tincture of hop. This last combination, or chloral, may be taken at bedtime in such insomnia, but a course of alkaline waters, or potash and buchu, with a non-nitrogenized diet, is also most desirable. The elimination of the already existing uric acid by such remedial measures, and the arrest of its further production by a suitable dietary, will be found most effective in the alleviation of the sleeplessness of the lithæmic—a sleeplessness which is very intractable to ordinary hypnotic measures. There seems much reason to suppose that the persistent insomnia of the more affluent classes, which impels them to resort to the most powerful and dangerous hypnotics in order to procure sleep, has in it an element of gout which is not recognised, and which thwarts the measures ordinarily effective and operative; so that remedies of unwonted potency or in unusual doses are necessary in order to secure sleep, or rather unconsciousness.

A totally different form of sleeplessness is found along with cerebral anæmia. Here there is sleepiness felt during the day, and especially when the upright posture is assumed; but at night, on lying down, instead of the inclination to slumber becoming more pronounced, a state of wakefulness is instituted. Hammond explains this on the ground that the brain is anæmic, and consequently lethargic, when the skull is the highest point in the organism, and the blood falls away into the unfilled vessels by the force of gravity; but that, when the horizontal posture is assumed, the head ceases to be the highest point and then the blood returns to it, fills the intracranial vessels, and a state of wakefulness is induced. These patients could sleep when going about their duties in the day, but they cannot sleep when they go to bed at nights. Such a condition is found in a most pronounced form in melancholics. It has been found that in such patients bromide of potassium, or chloral, however effective in procuring sleep, are unsuitable and indeed mischievous from their tendency to aggravate the condition of anæmia. When administered to the convalescent melancholic they retard recovery, and when given in the earlier stages tend to produce still further "brain starving," and to transfer the condition from one of temporary melancholia into the more advanced and permanent condition of chronic dementia. Both Crichton Browne and Hammond decry the resort to the above-named narcotics in cerebral anæmia, and advocate instead the use of alcohol and opium. These latter agents first dilate the cerebral vessels ere producing contraction of them, and so relieve the condition of gloom ere the patient falls asleep. Nor is the condition of cerebral anæmia induced by them so persistent and mischievous in its after-consequences as that produced by chloral and bromide of potassium. In the sleeplessness of cerebral anæmia, alcohol and opium may be given alone or combined at bedtime, for the purpose of procuring sleep; but their administration must be accompanied by a generous and liberal dietary, and by the exhibition of iron and of quinine in fair quantities. By such means the brain recovers its normal vascularity, and the sleepiness of the days and the sleeplessness of nights are exchanged for the usual sensations.

In order to give tone to the unfilled vessels digitalis—though it raises the blood-pressure—may often be given with advantage along with the quinine and iron. There is nothing inconsistent about the use of quinine under such circumstances.

Sleeplessness is, of course, a consequence of cerebral hyperæmia. We have already considered its association with conditions of active hyperæmia, accompanied by general vascular excitement, and the appropriate measures for its treatment. There is, however, another form of cerebral hyperæmia, in which insomnia is a prominent feature, that calls for remark. This is a form of passive hyperæmia from partial vaso-motor paralysis of the intracranial blood-vessels, usually associated with mental overwork. It is described by Niemeyer as a condition in which the mental disturbance is so great that it simulates melancholia, or mania. There is restlessness, anxiety, a general sense of dread, and such patients "cannot stay long in one place, go about restlessly, are worried, and are conscience-stricken about slight trifles." This last is a very significant statement, and throws a light upon some unfortunate actions otherwise apparently inexplicable. In speaking of the accompanying insomnia, Niemeyer writes: "In such cases the sleeplessness is almost absolute; opiates have no effect, or, after the exhibition of this remedy, which is injurious and dangerous to the patient, there is a short restless sleep, from which the patient awakes with all the symptoms increased." In such conditions it is obvious that the line of treatment to be pursued is one which will rest the excited and irritable brain, and in doing so embrace the condition of the blood-vessels, which is so important a factor in the malady. That a brain overtasked and approaching exhaustion should show great irritability, is in strict accord with what we know of the symptoms of commencing nerve-exhaustion. That the wearied brain should become hypersensitive about trifles is also in unison with our experience, and that the mental symptom—a morbidly exacting conscience, should manifest itself, is what we may readily believe. In such a state the person is indeed to be pitied, for all causes of pain or trouble are magnified, while the power to bear up against them is lessened. Nor is the suicide which occasionally gives a shock to society unintelligible, when we consider the number of individuals, and those too the most sensitive, who are reduced, more or less pronouncedly, to this pitiable condition. In estimating the effects of worry, of anxiety, of over-sensitiveness upon any individual, we must not either overlook or underestimate the peculiarities and capacities of such individual. What may be sufficient to utterly break down one person, may be borne by another with comparative equanimity, or even perhaps with ease. But because such is the case, it does not follow that the burden has not been too great for the person who succumbs under it.

What the line of treatment to be pursued under these distressing circumstances should be now calls for our attention. Niemeyer has told us that opiates are injurious. The vaso-motor paralysis which follows the use of this agent evidently aggravates the already existing condition. Chloral produces cerebral anæmia, but from its enervating effects upon the circulation it tends to make the general condition worse, while it affords temporary relief. It is not merely a question of regulating, so far as may be possible, the intracranial circulation; the condition of the cerebral cells calls for attention. A great portion of the hyperæmia is due to the persistent call of these cells for blood, until passive congestion, from vaso-motor paralysis, is induced. It is obvious then that one constituent factor of the treatment must be to lessen this cerebral activity by some suitable agent. Of all our remedies for such end, probably bromide of potassium is the most efficient. It leads to cerebral anæmia chiefly by its action upon the cerebral cells, lessening their activity and so diminishing their call for blood; while it exercises some sedative effect upon the circulation. With it digitalis may be advantageously combined. The efficacy of such combination, in cases of cerebral disturbance with a feeble circulation, is no longer a matter of hypothesis, it is an ascertained fact. Doubtless in such cases it is the action of digitalis upon the peripheral vessels rather than its effect upon the heart which is valuable here. The action of digitalis in contracting the terminal vessels is pronounced, and such action is very useful in the cases under discussion. Very possibly the allied action of ergotine may make it equally serviceable; and the use of ergotine in asylum practice to control cerebral hyperæmia is on the increase. By such a line of practice, then, we secure a condition of comparative inactivity in the cerebral cells, and also give tone to the enfeebled encephalic vessels. But while doing so, the demands upon that brain should be reduced to a minimum, and all outward and extrinsic sources of disturbance should be, as far as is practicable, done away with. As complete rest as is attainable is an important matter. Then again the blood itself should contain fair quantities of nutritive material, in order that the nutrition and restoration of the exhausted cerebral cells may be achieved and augmented. For this end liberal supplies of easily assimilable food, some generous wine, and iron, along with quinine, or even strychnine, should be taken. There is no inconsistency in giving quinine and strychnine, powerful nervine tonics, during the day, together with hypnotics at night—certainly not. By the combination of such measures good results may be expected, with more or less confidence, according to the state of the patient, the original constitution, and the conditions of the environment. The treatment, however, requires time, and compliance with it on the part of the patient. If it be impossible, or be thought to be impossible, to lessen the demands upon the brain, and this latter will but too easily and readily occur in the abnormal conscientious state which now obtains,[1] the treatment will be unsatisfactory; and then the patient will usually resort to those potent measures by which sleep, or rather unconsciousness, may be certainly induced, and will sooner or later pay the price for it.

When, however, in addition to the condition just described, there is added an element of lithæmia, and the cerebral cells are further irritated by the products of nitrogenized waste, then the case becomes trebly difficult to manage. The tendency to such complication is furthered by the resort to stimulant nitrogenized food by such persons, in order to increase their strength and improve their condition; a very natural error. In many cases, too, in addition to this excessive consumption of albuminoid matters, there is imperfect elimination of waste matters from the small quantities of fluid imbibed, which are insufficient to that thorough washing-out of the tissues, so desirable when there exists an excess of effete matter. This latter is especially the case with ladies, whose social requirements cause them to imbibe as small a bulk of fluids as is practicable. In such cases it is necessary, in addition to what has just been stated, to give the patients free supplies of alkaline waters, as Vals, Vichy, Carlsbad, Seltzer, or others, so as to at once secure the quantity of fluid necessary, together with the formation of soluble urates, which will readily find their way out of the organism by the natural emunctories. Without attention to this factor in the case, the other remedial measures will be but inefficient, or even futile.

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[1] Oliver Wendell Holmes, with his graphic and happy descriptions, tells us in *Elsie Venner*, what a terrible and exacting affair a sick or morbid conscience can be.

Having thus reviewed the different forms of sleeplessness, except perhaps that of acute mania, which scarcely comes within the sphere of this article, something may now be said about the chief forms of hypnotics in common use. They cannot be discussed at length, such treatment of the subject would be out of place here: the chief indications only can be given.

To take opium first. Its use is rather indicated in conditions of insomnia which take their origin in pain. When there is vascular excitement present, it is desirable to combine with it direct depressants of the circulation, as aconite or antimony. The subsequent cerebral anæmia induced by the resort to opium is not so pronounced as is that induced by chloral.

Hyoscyamus takes its place alongside of opium, and may be resorted to in cases where opium or morphia disagrees, as in cases of chronic renal disease. For these last class of patients the tincture of hop is often very serviceable, though now rarely prescribed; it is a very satisfactory agent in such cases.

Hydrate of chloral is comparatively valueless in sleeplessness due to pain, and is inferior, in this respect, it is said, to the croton-chloral-hydrate. It is, however, very useful in conditions of vascular excitement, either alone, or in combination with opium. In the delirium of acute pyrexia in children it may be usefully combined with the bromide of potassium. In cases of sleeplessness where there is a sustained high blood-pressure, or where there is distinct pyrexia, chloral hydrate is the hypnotic *par excellence*. It is, however, decidedly to be avoided in cases where the inability to sleep is due to worry and to brain exhaustion. In such cases, as in melancholia, the cerebral anæmia which follows its use is most objectionable and mischievous. It amounts to "brain-starvation," in fact, and the persons so affected are reduced to a pitiable condition. The persistent resort to chloral-hydrate is most disastrous in its consequences, and the temporary relief afforded by it is not to be set against its after effects.

Bromide of potassium has a decidedly sedative effect upon the brain cells; and the cerebral anæmia produced by its administration is rather due to its sedative action upon the cerebral cells by which they attract less blood to themselves, than to its effects upon the circulation; though doubtless to some extent it does diminish the activity of the heart. Its special advantage lies in its utility, where cerebral activity is kept up by far away peripheral irritation, especially when that irritation lies in the pelvic viscera. It may be given alone, or with opium, or with chloral, according to circumstances; and may often be usefully combined with hyoscyamus in cases where opium is contra-indicated. Its constant use, however, leads to diminished brain activity, and to intellectual lethargy.

Chloroform is a most potent agent, and is rarely resorted to as an hypnotic until other means of attaining the desired end have failed. The dangers attendant upon its use are so great that it is only resorted to in dire necessity. It is, however, occasionally used as a narcotic by the profession, but more frequently by persons upon their own responsibility. This chiefly occurs in those subject to sudden and unendurable pain, when nothing but the narcosis of chloroform would be effective. Probably indeed in these cases, all other and less objectionable means of attaining relief have been tried and have failed. According to Claude Bernard by combining opium, or rather morphia with chloroform, the sensory nerves and centres are affected ere the intèlligence and the motor powers are much influenced. But with choroform alone all are equally and alike affected. The danger of chloroform inhalation lies chiefly in the risk of an overdose being taken; as unconsciousness creeps on the motor power is involved, and then the amount taken may be, and too often is, far beyond what was intended. In another communication in this number of the *Practitioner* will be found some account of a most ingenious apparatus, by which the supply of chloroform is cut off as soon as the motor power is impaired. If resort to chloroform inhalation cannot be avoided by certain sufferers, surely it is not objectionable from any point of view that the danger attendant thereupon be reduced to a minimum.

There is another hypnotic agent of undoubted potency, which cannot be overlooked in the present inquiry, and that is—alcohol. If there be any use of alcohol that is free from objection it is its use as a narcotic in certain conditions. With many persons a dose of alcohol at bedtime is the very best nightcap they could possibly resort to. The cases best adapted to its use are those where there is mental worry and anxiety. In such states the first effect of alcohol in removing gloom and substituting pleasing sensations for unpleasant thoughts is eminently useful. A series of pleasant mental images are brought up on the mental horizon by its means, in place of the *triste* and sombre subjects which before its use occupied the foreground of the consciousness; and with such agreeable objects uppermost, the secondary effects come on, and the patient is wrapt in a refreshing renovating sleep. Probably the evil after effects of alcohol, so used, are less than those of any other agent which would achieve the same end. Unfortunately, however, commonly the very persons for whom alcohol would form the best hypnotic are those most opposed to its use; and where a full dose of alcohol would constitute the best remedy that could be resorted to, prejudice prevents its employment.

So much for the ordinary narcotic agents in common use. Over and above these are some other means of ensuring sleep, which are not contained in the pharmacopœia. Nor are these measures of little avail; they might be more commonly resorted to with advantage.

The first of these is bodily exercise, especially out of doors. Many cases of sleeplessness, as our common experience tells us, are due to this cause. With a number of individuals the omission of their wonted walk is followed by a restless and uneasy night. In such cases it would seem that very probably there are a number of motor centres in the cerebral convolutions which are still in a state of high tension, highly charged and unrelieved by discharges. It is not difficult to conceive that under such circumstances there should be such activity remaining undischarged as shall seriously interfere with the quiescence of those centres which have not only been relieved by discharges, but which are in that state of irritability which is the forerunner of exhaustion. Muscular exertion would relieve this condition of matters, and so would tend to the induction of sleep; not only that, but according to Preyer, the products of muscular exertion are directly hypnotic in their action.[1] If such a view should be substantiated, then muscular activity will take a decided position in the ranks of the means by which sleep may be secured.

In addition to these measures already enumerated, there are the mental means of attaining repose. Just as worry, excitement whether of a pleasant or of a disagreeable nature, anxiety and other mental conditions do away with and destroy sleep, so psychical quiescence tends to establish it. Different individuals seek this quietude in various ways and by varied measures. Thus in many families it is customary to abstract the mind from the distracting affairs of life by music; and such diversion of the thought is commonly productive of a mental condition highly favourable to sleep. With others again, whose minds are cast in a sterner mould, family and private devotion advantageously occupy the close of each day. With them there is that putting away of earthly subjects, that communing with their Creator, and that casting of their cares upon Him, under whose protection they sleep in confidence, which "gives that peace the world cannot give;" and so, comforted and sustained, they fall asleep under circumstances where otherwise no slumber could be attained. Such diversion of the mind at the close of each day cannot be too highly commended from a physiological point of view. Whether persons or families make their selection on higher or lower grounds, this is not the place to discuss their choice.

Many contrivances to secure sleep have been devised which act through the mind. Of these the counting of sheep rushing through a gateway, or of their leaping from a height in single consecutive order; or the mental repetition of numerals; or of x, y, z, are the most common; when there is simple want of consentaniety in the different cerebral centres, or a certain amount of mental activity remaining unused up, such plan may be successful in inducing sleep. But sleeplessness is very often a much too serious matter for any such means to be efficacious, and its successful treatment usually entails some physiological knowledge of the nature of sleep, as well as a wide and comprehensive grasp of the different factors in each case and of the means of meeting and of combating them.

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[1] Preyer, *Centralblatt d. Med. Wissenschaften*, No. 35, 1875, has found that lactate of soda injected subcutaneously, or injected in large quantities, into an empty stomach, causes sleep, and that highly concentrated sugar solutions, or other substances which may give rise to the formation of lactic acid in the stomach or intestines, also produce sleep.

THE PHYSIOLOGICAL ACTION OF ALCOHOL.

BY T. LAUDER BRUNTON, M.D., F.R.S.[1]

(*Concluded from* p. 64.)

WE now come to consider the effects which alcohol produces reflexly on other organs through the nerves of the stomach before it has actually been absorbed.[2] The glow which is felt in the stomach after taking a glass of brandy diffuses itself so rapidly over the body that many authorities have considered that it could only do so through the nervous system. Others again believe that absorption takes place so rapidly that the warmth may quite well be and is due to the action of the alcohol on the heart and vessels after it has got into them. In this case, as in many others, it is probable that both parties are right, and the effect is due partly to the one cause and partly to the other. For if you wish to warm a man quickly who is shivering with cold you give him a glass of raw brandy, and you do not dilute it with a tumblerful of water. And yet, according to Dogiel,[3] the diluted spirit will be more quickly absorbed than the strong; and if the warming effect is produced by the alcohol only after it gets into the vessels the dilute spirit should act more quickly than the neat. The fact seems to be that the brandy at first increases the circulation and warms the man by acting reflexly on the heart and vessels through the nerves of the stomach,[1] and that afterwards the alcohol is very quickly absorbed into the blood, and keeps up the primary effect by its special action on the nervous system, and through it upon the circulation.

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[1] Read in part before the Medical Society of London.

[2] In reference to congestion of the stomach as a cause of appetite *vide* Beaumont, *Experiments and Observations on the Gastric Juice and Physiology of Digestion*, and Buchheim's *Arzneimittellehre*, p. 42. On the secretion of gastric juice and the effect of alcohol upon it, *vide* Beaumont op. cit., Bernarp *Physiologie Expérimentale*, vol. ii. p. 388, and Külhne, *Physiologische Chemie*, p. 28.

[3] Dogiel, *Pflüger's Archiv.*, vol. viii.

When a large dose of alcohol is swallowed at once, the person or animal very often falls down immediately in a state of perfect unconsciousness, and unless medical assistance be at hand may never awake. Sir Benjamin Brodie, who made several experiments on this point, attributed this condition to reflex action from the stomach upon the heart and vessels.[2] The irritation applied to the inside of the viscus by the alcohol had produced shock in very much the same way as a blow on the epigastrium would have done.[3] But when alcohol is injected into the veins

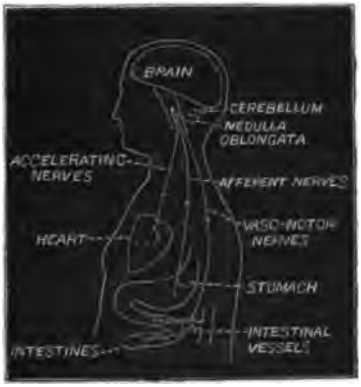

FIG. 1.—Diagram showing the reflex action on the heart and vessels of *large* doses of alcohol introduced into the stomach. The irritation produced by it is conveyed by the different nerves to the medulla oblongata, and thence by the vagus to the head, which it either slows or stops entirely. The mode of action on the intestinal vessels is not certain. It may simply arrest the normal action of the vaso-motor centre upon the intestinal vessels, or may be conducted down to them by vaso-inhibitory nerves, as represented in the diagram. In either case it will cause them to dilate.

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[1] Reflex contraction of the intestinal vessels probably occurs and drives the blood to the surface. Compare the experiments of Hermann and Ganz (*Pflüger's Archiv.*, vol. iii.), and of Meyer and Pribram (*Wiener Akad. Sitzungsber*, July, 1872), where cold drinks and mechanical irritation of the stomach raised the blood pressure, with those of Heidenhain (*Pflüger's Archiv.*, 1871, vol. iv. p. 1 to 119), where a rise in blood-pressure increased the flow of blood through the cutaneous vessels.

[2] Brodie, *Philos. Trans.* vol. ci. p. 179.

[3] *Vide* the author's article on "The Pathology of Shock and Syncope," *Practitioner*, vol. xi. p. 243.

the animals fall into much the same condition as when it is introduced into the stomach, and therefore several authorities have thought that Brodie's conclusions were wrong. They are, however, in great measure correct, for he found that a somewhat small dose of alcohol injected into the stomach of a cat knocked it down senseless, and in this condition it remained for about eight minutes. Then it recovered and walked about. There was no time here for the elimination or for the destruction of alcohol in the system, and consequently this effect which passed off so rapidly could not be due to the presence of the drug in the blood, and must be attributed to its action upon the stomach before absorption. But when larger quantities were given the animal did not recover in this short time. Then the primary shock lasted so long that before it passed off absorption had time to take place, the alcohol having found its way into the blood was carried by it to the nervous centres, acted upon them, and the shock passed into alcoholic coma.

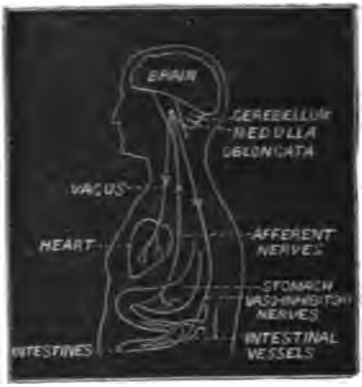

FIG. 2.—Diagram showing the reflex action upon the heart and vessels of moderate doses of alcohol. The irritation is conveyed to the medulla, as in Fig. 1, but instead of calling into action the vagus and vaso-inhibitory nerves, it excites the accelerating nerves of the heart, and probably the vaso-motor nerves of the intestines, thus increasing instead of diminishing the circulation in the body generally. This difference in the reflex action of large and small doses of alcohol upon the heart and vessels corresponds to the different action, already noticed, of slight and great irritation of the stomach, mechanical or otherwise—the slight stimulation increasing, and the great diminishing or arresting the circulation and secretion.

While large doses thus paralyze the heart more or less completely moderate doses stimulate it to act with increased rapidity, and at the same time with increased force. I consider this stimulating action upon the heart through the nerves of the stomach, even when no absorption has taken place, to be one of the most important properties of alcohol, for I believe it is reflexly in this way that we restore the circulation where it has nearly ceased by pouring a glass of brandy down a man's throat. When a person has been nearly drowned, or is dying from exposure to cold—when the pulse at the radial has ceased and the cardiac pulsations can hardly be perceived—we cannot imagine that absorption will go on very quickly from the stomach, and yet the good effects of the spirits we give quickly become evident.

Having said so much regarding the local action of alcohol on the stomach and its reflex action upon the heart through the nervous system, let us consider the effects it produces after its absorption into the blood. It must be constantly borne in mind that these effects are independent of those which alcohol produces reflexly, and may be antagonistic to them; so that after alcohol has acted on the nerve centres, it may prevent any farther reflex from the stomach.

Most authorities agree in saying that absorption takes place by the veins and not by the lacteals, but some consider it to occur chiefly in the stomach, others in the intestine. In all probability it occurs in both, and slight differences in the fulness of the stomach may alter the proportion taken up by it and by the intestine respectively. Strong alcohol injected directly into the blood causes coagulation, but this of course cannot occur when it is absorbed from the stomach. For if by any possibility it should enter the absorbing venous radicles in a concentrated state the coagulation it would there induce would at once bar its further progress.

It is therefore only with the action of alcohol more or less diluted on the blood and vessels with which we have to do. It acts on the white blood corpuscles by first increasing and then diminishing their amœboid movements. What the effect of such increased movements of the corpuscles will be upon the body as a whole it is difficult to say, but the result of its action on the red corpuscles is more easy to trace. George Harley[1]

[1] *Proceedings of the Royal Society*, vol. xiii. 1864.

and Schmiedeberg have found that it lessens their power of giving off oxygen, and must consequently more or less diminish the oxidation of the tissues. Now, both the functional activity of organs and the production of heat in the body depend on the processes of oxidation within them, and it is obvious that any interference with these processes is not likely to be beneficial so long as they are going on in a healthy way, and not too rapidly. As we shall afterwards see, however, this effect is to some extent counteracted, or even more than counteracted, by the action of alcohol in accelerating the circulation, and if the quantity taken be small, and not frequently repeated, little or no harm will ensue. If it be frequently taken, however, by persons in average health, and with fair digestion, its effects will become manifest in the imperfect combustion of fat, and its consequent accumulation in the tissues. This seems to occur especially in the skin, which acquires a velvety feeling. From this quality of the skin I have seen Professor Neumann of Vienna diagnose the potatory habits of a man whom one would otherwise never have suspected. If much saccharine or other fat-forming matters be taken at the same time with frequent doses of alcohol the subcutaneous tissue also becomes loaded with fat, as we so frequently see in brewers' draymen, and if the consumption of alcohol be excessive it causes fatty degeneration of various organs. This power of alcohol to lessen oxidation, useless or even injurious in health, increases the value which its other properties give it in the treatment of febrile diseases, where oxidation is going on too quickly, and rapidly destroying the tissues. The very increase of temperature which this oxidation causes helps of itself to accelerate this disintegration of the tissues, for a high temperature causes them to split up, even although they do not undergo oxidation. Thus the albuminous tissues probably become decomposed and yield urea, other nitrogenous substances, and fat. The fat does not undergo complete combustion but accumulates in the tissues from which it has been formed, and thus the heart of patients who have died of pyrexial diseases, instead of being purely muscular, is generally to a great extent fatty.

In such a condition of pyrexia alcohol will diminish the excessive waste in two ways. Firstly, it will impede oxidation, and secondly, by thus lessening the temperature, it will diminish tissue-disintegration.

But while alcohol thus modifies the blood, does it undergo no change itself? Does it simply course through the vessels for some time until it can be eliminated unchanged by the various emunctories, or does it undergo combustion in the blood as the grape sugar from which it is derived would do, and thus deserve like it the title of food?

Great numbers of experiments have been made to decide this question, and diametrically opposite opinions have been founded on them. Liebig classed it as a food along with sugar and starch, and no doubt was thrown on the correctness of this classification, until Lallemand, Perrin, and Duroy published their experiments, from which they concluded that alcohol is entirely eliminated in an unchanged condition, and can therefore, in no sense be termed a food. Their conclusions, however, were much more general than their experiments warranted, and they did not pass unchallenged. What their research actually showed was not that the whole of the alcohol injected passed out of the body, but only that a part of it is excreted. Similar experiments were made by Baudot, who instead of using large doses used small ones, and he found that instead of the whole or a great part of the alcohol being excreted, only a small fraction, so small as to amount practically to nothing, found its way out through the kidneys. The question was then taken up by the late Dr. Anstie, who did much to solve it; and had he only lived to complete the researches on which he was engaged at the time of his death would have settled it completely. His experiments, as well as those of Thudichum, Dupré, and Schulinus confirm Baudot's, and show that only a trifling fraction is eliminated. A year or two ago, Subbotin published some experiments, in which he found a much larger proportion of the alcohol to be excreted than the other observers just mentioned had done, but he, as well as Lallemand, Perrin, and Duroy, used very large doses. Now we all know that grape sugar is a most valuable food—the food we may say *par excellence* of the body, for others are converted into it in the liver—and in moderate quantities it undergoes complete combustion in the body, and is not eliminated in the urine. But a man may be rendered temporarily diabetic by giving him a large quantity of syrup at once, for the organism not being able to consume more than a limited amount at a time, the excess is thrown out by the kidneys. It is therefore not to be wondered at that alcohol should be excreted after large doses have been taken; in fact, the wonder would be if it were not.

The importance of the question whether alcohol undergoes oxidation in the body or not consists in this: if it is oxidized it will supply energy for muscular exertion, or for keeping up the animal heat, or for both, and will therefore be entitled to rank as a food, while if it is excreted unchanged it will have no claim to the name and must be classed with such substances as the organic alkaloids, which after acting on the nervous and muscular systems, while they are circulating in the blood, pass out after a while by the emunctories. It is the merit of Baudot, Anstie, and others who have worked at this subject, to have shown that alcohol is oxidized, and is thus to be reckoned as a food and not merely as a drug. But still more satisfactory evidence of its claim to the title of food is afforded by the fact that it will keep up the weight of the body and prolong life when the supply of other food is insufficient or is entirely wanting.

Dr. Hammond found that when he took an insufficient diet and was daily losing weight, the addition of alcohol not only prevented this loss of weight, but converted it into an actual gain.[1] In his work on "Stimulants and Narcotics," Dr. Anstie has collected a number of cases in which persons have lived for a considerable time either upon it alone or along with a quantity of food so small as to have been utterly inadequate without it.

From a survey of all the evidence on this subject, I think we may conclude that in moderate doses alcohol undergoes combustion in the body, and will supply energy, yield warmth, and tend to sustain life in the same way that sugar would do, and is therefore to be reckoned as a food. At the same time it has a power of diminishing oxidation which prevents its employment as a food to any great extent in health, but greatly increases its utility in disease.

In feverish conditions it diminishes tissue waste, and thus keeps up strength in three ways:—1. It undergoes combustion itself as a food instead of the tissues. 2. It lessens oxidation in them. 3. It lowers the temperature which itself increases tissue degeneration. It may perhaps seem rather contradictory to say that it undergoes combustion and yet diminishes combustion, but in this respect we may compare it to the sulphur which some people are accustomed to throw into their grate when the chimney takes fire—the sulphur burns itself, but it puts out the blazing soot.

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[1] Hammond's *Physiological Researches*, p. 55.

We now come to the action of alcohol upon the heart and vessels, and in order to prevent any complication from the reflex action of which we have already spoken, let us suppose that instead of pure brandy dilute spirits or some light wine has been taken, which will have little or no irritating effect upon the gastric mucous membrane. One of the best possible opportunities of studying the earlier and slighter effects of alcohol is afforded by a public dinner. If we look at our own hands or those of our neighbours before going in, especially if the ante-room is somewhat cold, we may find them somewhat pinched looking; the colour somewhat dusky and distributed in patches instead of being uniform; the veins very thin, almost like threads. They are of a somewhat dark blue colour, and on emptying them by pressure they fill very slowly, showing that the circulation is languid. After a few glasses of wine, however, their appearance begins to change. The hands now assume a uniform rosy tint, showing that the capillaries are now dilated and filled with bright arterial instead of dark venous blood; the veins swell up, become prominent, of a light blue colour almost like arteries, and when emptied by pressure fill rapidly, showing that the circulation has become very quick, and that they, like the capillaries, are now filled with blood which is tolerably bright, if not quite arterial, instead of the dark blood they previously contained. The hands entirely lose their shrunken look, little wrinkles in the skin disappear, indeed the hands become larger than usual, or, as my neighbour at a dinner table once expressed it, they become "podgy," from the amount of blood and intercellular fluid in the vessels and tissues; and rings previously loose become almost too tight. This dilatation of vessels, so readily seen in the hands, is not confined to them, but occurs generally throughout the body. The warm blood pouring from the interior of the chest and abdomen over the surface imparts to it a pleasing glow, and a most agreeable feeling of comfort prevades the whole frame. The face shares the general flush, and the pulsation of the temporal arteries not unfrequently becomes easily visible. The current of blood throughout the body is more rapid than before, and this rapidity does not depend simply on the dilated vessels offering less resistance to the current of the blood. No! the alcohol has stimulated the accelerating nerves of the heart, which cause it to pulsate not only more rapidly but more powerfully so that in animals, despite the dilatation of the vessels, the pressure has been found to rise in the arteries.[1] Here we have one at least of the most important conditions for the nutrition of all the tissues. The slight diminution in the oxidizing power of blood which alcohol occasions is many times over compensated by the amplitude of its current, and, as this is flowing rapidly through the tissues, bringing them new food and carrying off their waste products, is it any wonder that they work with more than usual vigour? The muscles acquire new strength; the work which previously fatigued them is done with ease; the mental faculties become much more acute, and new ones, previously unsuspected, may even appear. The merchant will be able to drive a harder bargain, the student will solve the problem which previously baffled him, the man who tries with difficulty to speak in a foreign language finds his stammering disappear and his tongue run on with ease, the melancholy man may sing a merry ditty, and the sedate man, whom no one would ever have suspected of such a thing, may succeed in making an excellent joke. Provided the liquor has been good, or, in other words, provided the alcohol employed has been free from all injurious admixture, all these effects, I believe, may be produced, and may pass away without any bad effects.

In the preceding lines I have sketched the action of alcohol in its fairest aspect, but its effects are not always so pleasant; for not a few persons, instead of becoming more bright, lively, active, and intelligent after taking a little alcohol, become heavy, sleepy and stupid. These different effects are partly dependent on the different constitution of the individuals, and partly on the quantity and kind of the alcoholic beverage. We find that the same differences exist in the effect of walking exercise upon the mental powers. Exercise, like alcohol, both dilates the vessels and increases the action of the heart. The mental processes of some persons go on very slowly when they are walking, and if they are asked a question they stand still to think and answer it.[1] They can think still better when they sit, and their brains are perhaps yet more active if they lie down. Others, again, do their brain-work more easily when walking about, and so instead of remaining at their desk they pace the room incessantly while thinking, and only take the pen in their hand when they are ready to write. At first sight, it seems odd that the conditions most favourable for thought should be so different in two persons, and yet when we come to examine them more closely we find that both persons, in different ways, are seeking the same thing, viz., a fuller supply of blood to their brains. Position has a good deal to do with this, the head receiving more blood when it is lowered than when it is high. Almost every one unconsciously shows this by bending forward instead of sitting upright when engaged in lively conversation, and more especially in debate, when the greatest possible mental activity is desired. The influence of position also makes itself felt, in the kneeling posture—for example, during prayers in church, where sometimes, very much against the will of the individual, thoughts utterly unconnected with the devotions in which he appears to be engaged whirl through his brain, new projects are formed and problems solved with a rapidity surprising to himself. In some persons whose blood-vessels are lax, either from natural constitution or in consequence of debility or exhaustion, the recumbent posture, which allows a free current of blood to the head, is the most favourable for thought.[2] Now both exercise and alcohol have the effect of dilating the vessels, and, at the same time, of increasing the action of the heart. In some persons the relaxation of the blood-vessels caused by exercise is greater than the stimulation of the heart, so that, although this organ is beating somewhat more vigourously, the dilated vessels of the body draw away the blood and leave the brain more anæmic than before. Consequently such persons do not think so well while walking, and they may be rendered rather stupid than lively by alcohol. Others again, in whom stimulation of the heart is more easily induced than relaxation of the vessels, either by exercise or alcohol, so that the rapid and powerful cardiac pulsations increase the current of blood to the brain more than the dilated vessels can diminish it, think well while walking about, and have their mental power increased by alcohol.

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[1] Dogiel, *Pflüger's Archiv.*, vol. viii. p. 605.

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[1] It must be remembered that the effects here discussed may be due in great measure to diversion of nervous energy (*vide* the author's article on "Inhibition Central and Peripheral," *West Riding Asylum Reports*, vol. iv. p. 210), and not entirely to altered distribution of blood.

[2] This is, however, often interfered with by some other factor, possibly some reflex action from the skin of the head, which induces sleep in this position.

A moderate quantity of alcohol may thus enable a man to overcome a sudden difficulty, but can its effects be kept up so as to help him with a prolonged effort? Does the alcohol supply new strength, or does it merely enable a man to use up his reserve of energy? If it really supplies strength we ought to find it doing so each time it is administered; but if it merely helps to use up reserve energy, we will find that each successive time it is given the organism responds less and less readily to the call, just as a man gives more and more grudgingly at each successive demand upon his purse. Now the question was thoroughly tested during the Ashanti campaign, and the following are the results as recorded by Professor Parkes:[1]—

"The first effect of alcohol, when given in a moderate dose (for example, what is equal to one fluid ounce of absolute alcohol), is reviving, but this effect is transient. The reviving effect goes off after at the utmost two and a half miles of additional march, and sometimes much before this; then the previous languor and sense of exhaustion not only return, but are sometimes more intense, and if alcohol is again resorted to, its effects are now less satisfactory. Its reviving power is usually not so marked, and its peculiar anæsthetic and narcotising influence can often be distinctly traced. The men feel heavy, dull, disinclined to march, and are less willing and cheerful."

From this it is evident that alcohol does not impart strength, but rather enables a man to use up in a short time the energy which he usually would have taken a much longer time to expend. If he only requires to make a single effort and can rest afterwards until he has replaced his exhausted store, the additional temporary strength obtained by using alcohol may enable him to overcome an obstacle which would otherwise have baffled him, but if he has to make prolonged exertions alcohol is injurious.

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[1] *On the Issue of a Spirit-Ration during the Ashanti Campaign of* 1874, p. viii.

Now the heart seems to be affected by alcohol in the same way as the body generally. No new strength is imparted to it, but it is enabled to draw on its reserve. Thus Parkes has found that when brandy is given to healthy men the pulse becomes quicker, but after the effects of the brandy have passed off it becomes slower than natural, so that the number of pulsations and amount of work done by the heart in twenty-four hours is much the same whether brandy have been taken or not, unless the doses be large and repeated.[1]

The question therefore at once arises,—Is alcohol only useful in stimulating the heart to do additional work, and thus averting the danger of failing circulation for a short time only, or can it be used in diseases where this danger is to be averted for days, and even weeks, together? Before attempting to answer this question, I would remind you that a heart which is beating more quickly than usual wears itself sooner out than one which is acting slowly. It has been found that a heart which has been made to pulsate slowly for some time by irritation of the vagus, will continue to beat for a good while after it has been removed from an animal's body; while, on the contrary, it very soon ceases to beat if the vagi, instead of being irritated, have been cut, so as to allow the pulsations to be very rapid for some little time before the animal's death.[2] What is observed in these excised hearts is only an exaggerated representation of what occurs in the body, and although in it the pulsations may continue days instead of minutes, yet the final result will be similar. If alcohol always quickened the pulse in disease as it generally does in health it would probably be injurious in prolonged illness, as it was found to be in prolonged exertions by the soldiers in Ashanti. But this is not the case, for in fever the quick pulse frequently becomes slower after the administration of alcohol, and, indeed, an excellent rule of practice is not to give alcohol if it increases the rapidity of the pulse already too quick. Alcohol thus economises the vital power of the heart, and tends to prevent death from exhaustion. It is difficult to say precisely how the slowing of the pulse is effected. Probably it is due partly to stimulation of the vagus, for alcohol stimulates this nerve as well as the accelerating nerves of the heart, and partly to the alcohol increasing the power of the weakened vaso-motor centre, either by acting upon it directly or by increasing the supply of blood to it, and thus giving greater tone to the vessels, and raising the pressure in them. It may be also partly due to the action of the alcohol in lowering temperature. For heat is a stimulant, and cold is a sedative to the ganglia of the heart both in and out of the body, and other things being equal, its pulsations will be quick or slow according as the temperature is high or low.[1] And just as hearts which have been beating quickly in consequence of division of the vagi, soon lose their vitality, so do hearts which have been pulsating rapidly in consequence of heat, soon cease to beat, while those which have been exposed to a lower temperature and have been beating more slowly, retain their vitality for a considerable time.[2]

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[1] Parkes, *Proceedings of the Royal Society*, No. 150, 1874, p. 190.

[2] Czermak and Piotrowsky, *Wiener Akad. Sitzungsberichte*, xxv. p. 431.

The power of alcohol to reduce the animal heat is assisted by its property of diminishing oxidation, but is chiefly, no doubt, due to its action upon the vascular system. As I have already mentioned, it produces a rosy flush and a glow on the skin by dilating the cutaneous vessels and allowing the warm blood from the interior of the body to circulate freely through them. This agreeable warmth is popularly believed to be due to increased production of heat in the body, and persons leaving a warm room for a cold walk or drive will often take a glass of spirits "to keep out the cold." But alcohol instead of really heating the body, only warms the skin at the expense of the heart, lungs, and intestines, and so really lets in the cold to these organs from which nature tries hard to exclude it. For, in a healthy man external cold causes all the vessels of the skin to contract, so that very little blood flows through them, or none at all. The skin itself thus becomes cold and blue, but the deeper structures retain their normal warmth, for heat passes from them to the skin very slowly indeed by simple conduction, and it is the circulation which maintains among them an equality of temperature. When alcohol is taken, however, this arrangement is disturbed, the cutaneous vessels instead of contracting become dilated, and the blood pouring through them warms the cold skin comfortably. But in doing this it loses heat itself; it returns to the heart at each revolution a little colder than it left it; soon the temperature of the whole mass of blood and of the internal organs becomes reduced, and the last state of that man is much worse than the first. Where men are subject to great and prolonged exposure to cold, experience has taught them the danger of taking spirits while the exposure continues. My friend Dr. Fayrer told me that when crawling through the wet heather in pursuit of deer on a cold day, he offered the keeper who accompanied him a pull from his flask. The old man declined, saying, "No, thank you, it is *too cold.*" The lumberers in Canada who are engaged in felling timber in the pine forests, living there all winter, sleeping in holes dug in the snow, and lying on spruce branches covered with buffalo robes, allow no spirits in their camp, and destroy any that may be found there. The experience of Arctic travellers on this subject is nearly unanimous; and I owe to my friend Dr. Milner Fothergill an anecdote which illustrates it in a very striking way. A party of Americans crossing the Sierra Nevada encamped at a spot above the snow line, and in an exposed situation. Some of them took a good deal of spirits before going to sleep, and they lay down warm and happy; some took a moderate quantity, and they lay down somewhat but not very cold, others took none at all and they lay down very cold and miserable. Next morning, however, those who had taken no spirits got up feeling quite well, those who had taken a little got up feeling cold and wretched, those who had taken a good deal did not get up at all, they had perished from cold during the night. Those who took no alcohol kept their heart warm at the expense of their skin, and they remained well; those who took much warmed their skin at the expense of their heart, and they died.

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[1] Panum, *Bibliothek für Lager*, Bd. xi. p. 468, and Schmidt's *Jahrb.* 1858. For other references, and for some original experiments on this subject, see the author's article on "The Influence of Temperature on the Mammalian Heart, and on the Action of the Vagus," *St. Bartholomew's Hospital Reports*, vol. vii.

[2] Panum and others, op. cit.

But while alcohol is thus injurious during prolonged exposure to cold, the case is very different after the exposure is over, and its administration may then be very beneficial. Supposing a man after being out all day comes home much chilled to a warm fireside. He stands before the grate and turns himself round and round, but he cannot get himself warmed through. The cutaneous vessels so long contracted by the cold will not relax all at once, and the deeper tissues gain heat very slowly, just as they very slowly lose it by mere conduction through the skin. If a little spirits be now taken and especially if it be taken hot, the cutaneous vessels dilate, allow the blood to circulate through them and become warmed by the fire, it returns warm to the internal organs, and soon the whole body is in a pleasing glow. At the same time the dilatation of the cutaneous vessels opens new channels to the blood which has been pent up in the interior of the body and thus lessens any tendency to congestion or inflammation of internal organs, so that a glass of hot brandy and water at the proper time may possibly prevent a bronchitis or pleurisy. Here I may just mention that although alcohol during continued exposure is generally injurious, yet in some instances where pain or cramp in the internal organs seem to indicate more risk from their engorgement than from diminution of the general temperature of the body, it may be beneficial, even while the exposure continues.

The dilatation of the vessels produced by alcohol has other consequences than equal distribution of heat between the surface and interior, for the dilatation does not occur equally in every vascular district. Generally the vessels of the brain are especially dilated, as is seen both from the mental activity usually manifested, and from direct observation of the vessels themselves; but sometimes those of other parts, probably those of the intestines, would seem to be more particularly affected, and the blood being thus drained away from the brain, it becomes anæmic and sleep ensues.

We have now to consider the effects of alcohol when given in such quantities as to evidence its poisonous qualities and produce intoxication. Excepting when the dose is so excessive as to produce shock, the symptoms of intoxication are always preceded by those of stimulation already described. It is (as I have already observed) very difficult to say how far the stimulating action depends on the increased circulation through the nervous centres only, or how much of it may be due to the action of the alcohol on the nervous structures themselves. The symptoms of intoxication must, however, be referred to a paralyzing action of the alcohol on the nerve centres, for although as intoxication progresses a diminution in the activity of the cerebral circulation occurs, and the well nourished brain becomes anæmic, this alone is insufficient to account for the effects we observe. The first of these are weakening of the mental faculties and of the power of co-ordination. The higher faculties seem to go first, and a man's judgment becomes impaired while his memory and imagination are still more lively than usual. Then these faculties diminish and the emotions become more prominent, so that a man is either ready to swear eternal friendship all round, or becomes as anxious for a fight as an Irishman at Donnybrook; is gay, mirthful and hilarious, or subdued and lachrymose, melting into a flood of tears without any apparent cause.

At the same time that the cerebral faculties are disappearing one after another, the power of co-ordination becomes impaired. This is most evident in the tongue and legs, the speech becoming thick and indistinct so that the pronunciation of the words "British Constitution," becomes next to an impossibility, and locomotion becomes staggering and uncertain. Although loss of the mental faculties and loss of co-ordinating power generally go hand-in-hand, yet either of them may occur a good while before the other, so that persons who seem stupefied by drink may rise and walk with the utmost steadiness, while others who seem perfectly unaffected while sitting and can discourse on any subject with freedom will find great difficulty in steering their way from the table to the door. In popular language, one man, is said to be drunk in his head and another in his legs. It is not, however, the legs that are in fault, as the drunk man himself well knows, but the nervous apparatus that directs them, and this in all probability is the cerebellum, as Flourens supposed it to be. This physiologist found that when he sliced away the cerebellum bit by bit the animals walked exactly as if they were drunk,[1] and on the other hand when he examined the cerebellum of drunk animals he always found it to be congested. The researches of my friend Professor Ferrier render it all the more probable that the cerebellum is the nervous centre on which this loss of locomotory power depends, for he has found it to be the centre for regulating the movements of the eyes and co-ordinating the motions of the body with them. Now double vision is one of the most marked symptoms of alcoholic intoxication, and the staggering of a drunk man seems to be dependent on erroneous conceptions of the position of surrounding objects, for he not unfrequently vehemently asserts that he is perfectly steady but everything else is drunk, and all his troubles are owing to an ill-disposed lamp-post which went out of its way to bump him, or an evil-minded pavement which rose up and hit him on the nose.

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[1] *Propriétés et Fonctions du Système Nerveux*, p. 827.

The cerebrum and cerebellum are thus the first parts of the nervous system to suffer, and even after their functions are completely abolished the spinal cord will perform its functions, and a man incapable of thinking, speaking, or walking, will be able to ride, the impression made on his legs by the saddle causing reflex contraction of his adductors and enabling him to sit tolerably firmly although the upper part of his body may be swaying helplessly about. At this time, however, the reflex action of the nerve centres regulating the heart's vessels is much impaired or almost entirely abolished,[1] and herein is one source of safety to the drunk man. For sometimes a person in this condition may be seen riding furiously along a road, the horse swerves or turns a corner quickly and the rider is pitched forcibly off. The bystanders rush up expecting to find him dead, but no, beyond a severe bruise or two, and perhaps some tear or cut, he is nothing the worse. The fall which would have killed a sober man has not hurt the drunk one, for the alcohol has paralyzed the nervous apparatus,[2] through which shock would otherwise have been produced. The medulla oblongata continues its functions after the cord also has ceased to act, but by and by it also succumbs; and if the dose be sufficiently large the respiration becomes weaker and weaker, and finally death ensues.

The motor ganglia of the heart are also weakened by the action of alcohol upon them, but, in general, death is due to stoppage of the respiration, and not of the circulation, except in cases where shock has been produced by enormous doses of alcohol swallowed at once.

To resume, the chief points in this paper are :—

1. Alcohol, in small quantities, increases the secretion of

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[1] *Vide* Dogiel, *Pflüger's Arch.* viii.

[2] *Practitioner*, vol. xi. p. 250.

gastric juice and the movements of the stomach, and thus aids digestion. Although unnecessary in health, it is useful in exhaustion and debility.

2. It increases the force and frequency of the pulse, by acting reflexly though the nerves of the stomach.

3. In large doses it impairs digestion by over-irritating the stomach.

4. It may produce death reflexly by shock.

5. After absorption into the blood, it lessens the oxidising power of the red blood corpuscles. This property renders it useful in reducing temperature; when constantly or very frequently present in the blood, it causes accumulation of fat, and fatty degeneration of organs.

6. It undergoes combustion in the body, maintains or increases the body weight, and prolongs life on an insufficient diet. It is therefore entitled to be reckoned as a food.

7. If large doses are taken, part of it is excreted unchanged.

8. It dilates the blood-vessels, increases the force and frequency of the heart by its action on the nervous centres to which it is conveyed by the blood, imparts a feeling of comfort, and facilitates bodily and mental labour. It does not give additional strength, but merely enables a man to draw upon his reserve energy. It may thus give assistance in a single effort, but not in prolonged exertions.

9. The same is the case with the heart; but in disease alcohol frequently slows instead of quickening the pulsations of this organ, and thus economises instead of expending its reserve energy.

10. By dilating the vessels of the skin, alcohol warms the surface at the expense of the internal organs. It is thus injurious when taken during exposure to cold, but beneficial when taken after the exposure is over, as it tends to prevent congestion of internal organs.

11. The symptoms of intoxication are due to paralysis of the nervous system; the cerebrum and cerebellum being first affected, then the cord, and lastly the medulla oblongata. It is through paralysis of the medulla that alcohol usually causes death.

12. The apparent immunity which drunken men enjoy from the usual effects of serious accidents is due to paralysis of the nervous mechanism, through which shock would be produced in a sober condition.

ON THE USE OF SUBCUTANEOUS INJECTIONS OF MORPHIA IN SPASMODIC ASTHMA AND OBSTINATE CONSTIPATION.

BY W. MCGREGOR BURNS, M.D., M.R.C.S., ETC.

DR. Keith Anderson's communication in the November number of the *Practitioner*, on the value of the hypodermic injection of morphia in cases of spasmodic asthma, although interesting, is not by any means new, as that method of treating spasmodic asthma has been practised by me since the spring of 1862.

My first case was that of a young lady in the town of Ayr, where I was then residing; the relief was almost instantaneous, and there was no return of the disease in her case until a considerable time after, when she had removed to a distance of about twenty miles inland from Ayr. I visited her there two or three times, and on each occasion she was immediately relieved by the morphia injection.

Since then I have used it very often, and almost, although not always, with immediate relief to the patients.

I may further state that I have frequently applied the same treatment over a course of several years, and with like success, in cases of obstinate constipation of the bowels, where the stomach rejected every form of purgative, and where enemata utterly failed. After a hypodermic injection over the region of the stomach of fifteen to twenty minims of the ordinary solution of morphia, vomiting ceased, rest was procured, and very soon the stomach received and retained castor oil or any ordinary purgative sufficient to move the bowels freely, and bring about the desired relief.

ON THE COMBINED USE OF MORPHIA AND ATROPIA IN THE TREATMENT OF SPASMODIC ASTHMA.

BY G. OLIVER, M.D., LOND.

THE communication of Dr. Keith Anderson "On the Treatment of Spasmodic Asthma by the Subcutaneous Injection of Morphia," in the *Practitioner* of November, calls the attention of the profession to the remarkable power possessed by subcutaneous morphia over asthmatic paroxysms. While the experience of Dr. Anderson is probably far from singular, it is surprising to find this valuable method of treatment without record in our literature. The agony of a severe paroxysm of asthmatic spasm has surely very often suggested to the practitioner a subcutaneous dose of morphia, which he has administered with the effect of quickly securing all the wished-for good results.

In 1871, while treating a very distressing case of spasmodic asthma, I became acquainted with the effect of the subcutaneous injection of morphia in this disease, and I have found it a safe and reliable remedy in other similar cases.

While regarding morphia as the principal agent in relieving asthmatic spasm, I have found the combination of it with atropia to act more quickly, and to combat the attacks more completely and effectually. Atropia seems to assist morphia not only in quelling the nervo-muscular storm, but in obviating the distressing and depressing after-symptoms of morphia narcosis which now and then are met with. Besides this, belladonna is now fairly established in the therapeutics of asthma chiefly through the advocacy of Trousseau, Seé,[1] Hyde Salter,[2] and G. Wood.[3]

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[1] *Practitioner*, July, 1869.

[2] *Lancet*, January 30th, 1869.

[3] *Philadelphia Med. Times*, September 19th, 1874.

We may look upon the subcutaneous use of the active principle—atropia—as of value at least equal to that of the ordinary administration of belladonna.[1]

There is still another reason why atropia may enable the asthmatic to derive more benefit and a safer relief from the combination of it with morphia than from morphia alone. Dr. Seé has shown (*Practitioner*, 1869) that belladonna quickens respiration, and, during the paroxysm, the altered respiratory rhythm of asthmatic breathing is changed to one of a different type; but, during the second stage, "the breathing becomes slower from fatigue of the pneumogastrics, and we have now a renewed indication for the use of belladonna, which will quicken respiration again. It is for this reason that you must not prescribe opiates, as is often done by physicians who are called in during the attack, and will insist on soothing the patient at any cost. Opium acts inversely to belladonna; it diminishes the frequency of breathing to the damage of the patient." While the pneumogastrics are exhausted, may not the injurious depression of morphia be checked by the stimulating power of atropia? Here the guarded use of opiates is a cardinal point of treatment; but to secure from them a relief from suffering without dangerous depression is a real gain.

But an appeal to experience is all-important:—

In July, 1871, a lady, aged 30, came under my care suffering intensely from spasmodic asthma of a somewhat bronchitic type, yet the neurotic element was very prominent. She had suffered for four years from very violent paroxyms, usually ushered in by slight bronchitis: each attack confining her to bed for a week or two, and the quiet intervals were too short to allow her to regain her strength properly. The lungs and heart were free from disease. Most of the ordinary remedies had been tried with only partial relief. The late Dr. Salter prescribed for her large doses of belladonna with ipecacuanha, which, affording somewhat more benefit than other drugs, were freely consumed in desperation by the patient who was hungering for relief at any cost. With dilated pupils, dry throat and foul tongue, the agonising spasm would only partially relax, and an unusually severe midnight paroxysm, which I was summoned to treat, would not give way at all. I injected a third of a grain of acetate of morphia;—in about ten minutes the breathing was calm and nearly free from wheezing. On awaking next morning nausea, vomiting, and headache, detracted somewhat from the joy of my patient at having discovered a remedy so effectual and so speedy in its operation. After repeating the injection with the same results, I combined with the morphia $\frac{1}{100}$ gr. of sulphate of atropia. Within a shorter period—not exceeding five minutes —the breathing was comfortable and free from wheezing; and next morning nausea, vomiting, &c. were not complained of. On one or two occasions I used atropia alone; the result being relief to the breathing in about ten minutes. It was quite clear the relief was much less decided than after morphia alone, and this was inferior to the combination of both remedies which set the patients breathing perfectly at ease for the night without the penalty of vomiting and headache in the morning. The subsequent history of this case has shown these points of experience:—

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[1] For a case of spasmodic asthma successfully treated by subcutaneous atropia, see *Journal de Médecine et de Chirurgic pratiques*, No. 5,747.

1. Morphia and atropia are superior to morphia alone; the good effect is more speedy and complete, and they produce no depressing gastric disturbance.

2. During the first year of subcutaneous treatment the asthmatic paroxysms were, as a rule, met by the injections as soon as relief was urgently demanded. The patient struggled on with her attacks before calling in her medical attendant who, only then, injected the remedies. While these attacks in mid-career were always quickly brought under control, they—as severe asthmatic paroxysms—notwithstanding the continued use of the injections, required time for complete and safe subsidence. While thankful for a means so effectual as this in quelling the violent storms, my patient sought for further benefit by attacking the earliest approach of them by the injections. This combined prophylactic and curative use of the remedies has been resorted to during the last three years, and, during this long period, there has not been one severe attack as in former years. At the very onset of bronchial disturbance, an injection night and morning for a few days together has usually kept the threatened asthmatic spasm in abeyance and apparently stamped out the

beginnings of an attack of the old type. Besides this, the patient asserts there is less susceptibility to bronchial attacks as well as the accompanying asthma, and the intervals between the attacks are lengthened, asthmatic breathing being often totally absent for a month or six weeks.

3. The very frequent use of the injections does not appear to injure the general health.

4. Speedy relief. The effect is generally declared in five minutes in the form of comfortable sleep and quiet breathing. The most intense attack has given way completely in from fifteen to twenty minutes.

This case shows that we may arrest in the earliest stage an attack of spasmodic asthma, thus preventing much suffering, removing the evil effects of the attack, and prolonging the intervals of ease. The nerve-bias, fostered by every attack, which ingrain more and more the "asthmatic habit," may be persistently and successfully resisted by the repeated subcutaneous employment of morphia and atropia.

Reviews.

Lectures on Dermatology; delivered in the Royal College of Surgeons of England, in 1874-1875. By ERASMUS WILSON, F.R.S., F.R.C.S., Member of Council and Professor of Dermatology. London: J. and A. Churchill, 1875.

THE Lectures published in this volume are in continuation of those delivered in preceding years by Professor Wilson, and contain much valuable matter in reference to the diseased conditions recognised in the author's nomenclature as Struma, Lupus, Lymphadenoma, Xanthoma, Epithelioma, Ichthyosis, Scleriasis, Verruca, Clavus, Nævus, Spargosis, Molluscum, Cheloma, and Prurigo. They are preceded by a description of the models which have been recently added to the dermatological collection of the College.

The various subjects discussed under these heads are illustrated by cases from the author's experience, and by a *résumé* of the important recent literature which has a direct bearing on them. The volume is interesting and instructive alike to the general practitioner and to those who devote themselves more especially to the study of skin diseases.

We observe that Professor Wilson draws a clear distinction between Lupus and the diseased condition named Lupus Erythematosus, fully bearing out in this respect the views held by Hebra's school. At the same time he differs from the Vienna dermatologists in regard to its pathology, holding that "the glands of the skin suffer most seriously only because they form a part of the structure of the corium, and are deprived of the usual facilities of escape of their accumulated contents."

Under the head of neurotic excoriations, Professor Wilson describes a form of skin disease of which he has several examples. It is characterised by a localised hyperæmia accompanied by intense itching, on which as a result of scratching an excoriation is produced. The oval or quadrangular spots disappear, gradually undergoing the usual changes of colour produced by pigmentation after blood has been effused in the corium. We can bear testimony to the fidelity of Professor Wilson's description of these peculiar cases, which now that they can be more easily identified will probably be found not to be so rare as has been imagined.

The Management of Eczema. By L. DUNCAN BULKLEY, A.M., M.D. New York: G. P. Putnam's Sons, 1875.

The Relations of the Urine to Diseases of the Skin. Ibid.

DR. BULKLEY'S views in regard to the nature of Eczema agree in the main points with those taught by English writers. He recognises a special constitutional vice as underlying the disease, about which however we get no more definite information than that "it is intimately associated with blood changes, represented in the main by a sub-oxidation or hyperacidity, as found in the stomach, kidney, &c., and this state, moreover, is closely allied to gout, rheumatism, and scrofula." His rules for the management of the disease are clearly expressed, judicious and practical. The treatise is short, but contains as much as is to be found in many of the larger monographs.

Dr. Bulkley shows by summary of a series of examinations of the urine of patients suffering from skin diseases, made by himself and collected from other sources, that in a large majority of such cases the excretion is abnormal. But the abnormalities observed are not sufficiently uniform to be of much value either as regards diagnosis, intimate pathology or treatment. The field is however a promising one, and Dr. Bulkley has done well to bring this subject prominently into notice.

An Introduction to Pathology and Morbid Anatomy. By T. HENRY GREEN, M.D. Third Edition. London: Henry Renshaw.

THIS useful and convenient manual has already reached a third edition, and we are glad to find that, although it has grown somewhat larger, it still remains a little book: and we are inclined to forgive the increase in size on account of the valuable additions which the author has made both to the printed matter and the illustrations. Most of the chapters have been extended. Some of them—such as those on "Inflammation of Vascular Connective Tissue," and on "Inflammation of the Blood-vessels and Heart" (in the second edition)—have been divided into two. "Leukæmia" has a chapter to itself instead of being treated along with the "Lymphomata;" and an entirely new chapter is devoted to the important subject of "Tissue Changes in Pyrexia. "An Account of the Pathology of Pulmonary Emphysema"—entirely omitted in the last edition—is now given; and the chapter on "Fatty Degeneration" is greatly improved, advantage having been taken of the light thrown upon its pathology by the researches of Voit and Bauer. A notice of Cohnheim's new observations has been induced into the chapter on "Inflammation;" and those on "Inflammation of the Kidneys and Lungs," among others, have been considerably enlarged. A number of new woodcuts have been introduced, and some of the old ones have been replaced by other and infinitely better ones—compare, for instance, Fig. 6 of the second with Fig. 12 of the present edition. The new illustrations, drawn by Mr. Collings from preparations by Dr. Green himself, are very good; and the care and trouble expended by the author in the preparation of this edition will no doubt increase the popularity of his book, great although it already is.

Diseases of the Nose. By SPENCER WATSON. London: H. K. Lewis, Gower Street.

THIS is a neat volume—well printed, well illustrated, and with a good index. Its author has spared himself neither time nor trouble. In the main, it is a compilation; but Mr. Watson, having gleaned all that he found good in other writers—English, French, and German—has added in many places valuable experience of his own. His descriptions are clear, and he writes in a correct and agreeable style. Yet we cannot speak of this work with quite unqualified praise. It consists of 472 pages. At what a rate are we being flooded with knowledge, if so small an organ as the nose must have so large a volume to itself. But when we look, we find not only its surgery, but also its anatomy and physiology, set down for us; yet the two latter are already within the reach of all who possess the ordinary hand-books. "Diseases of the Antrum" occupy fifty pages; but the antrum is no part of the nose; besides, its diseases have been already fully described by writers of distinguished ability and great experience. If we are to have a book on the Nose, including the Antrum, why not one on the Vagina, including the Uterus; or the Popliteal Space, including the Knee-Joint. Again, Mr. Watson sometimes strays beyond his particular subject into the realms of general surgery and pathology. He gives, too, an Appendix of Cases, which swallows up seventy-five pages. A writer may work either towards concentration or towards expansion. Mr. Watson has, we venture to think, leaned too much to the latter. His book is a good one, but in its next edition, if he will make it shorter, he may make it still better than it is.

Clinic of the Month.

Diabetes Insipidus treated by Belladonna and Ergot.—Dr. Murrell gives the details of a case in which the above remedies were successively tried. The patient was a boy of 15 years of age, in University College Hospital, under the care of Dr. Ringer. When six months of age he had the measles, and four or five months later it was noticed that he drank much more than is usual with children of that age. When about fifteen months old, he fell and cut his head against the stairs. Since that time he had suffered greatly from thirst. He was always drinking, and attempts made to keep him from drinking always made him sick and bad. His skin was dry and harsh, he never sweated, always seemed chilly, suffered from cramps in his legs. He was sick every morning. The mouth was dry, the lips were dry, and the bowels confined. The urine was clear, colourless, and sp. gr. 1,005—1,008, temp. 98°—99° F. The average daily evacuation of urine for five days after admission was five pints. Five drops of the tincture of belladonna were then prescribed for five days, then ten minims for seven days; then fifteen minims for four days; then twenty minims for eight days; then twenty-five minims for four days, and finally half a drachm for six days. This effect was steady diminution of the quantity of urine till the quantity ultimately fell to about two pints per diem. After ten days' absence from the hospital, during which he took no medicine, he was readmitted and the quantity of urine was found to be still further diminished. Some time after he reappeared, and the disease which had in the meanwhile reappeared was treated with ergot of rye. The patient was kept for some days without any treatment, and the quantity of urine passed was found to be about six pints per diem. The liquid extract of ergot was then prescribed in gradually increasing doses, beginning with fifteen minims, and rising to aggregate daily doses of two drachms, with excellent results, the quantity again falling to an amount but little above the normal. During a part of the period he was taking the ergot, quassia was prescribed. The pathology of the disease in this case at least appears to be that the blows on the head produced more or less paralysis of the vaso-motor centre in the medulla oblongata, and hence a freer flow of blood through the dilated capillaries of the kidneys than natural. Now Harley has shown that after the administration of a moderate dose of belladonna the arteries are contracted, though when a large dose is given the contraction is replaced by dilatation, and its beneficial effects in this case are sufficiently explained on that theory, and the same may be said of ergot. (*British Medical Journal,* Jan. 1, 1876.)

Treatment of Epistaxis by the Internal Administration of Ergot.—Dr. George St. George, of Lisburn, observes that the treatment of Epistaxis is often attended with great difficulty, especially in persons enfeebled by age. He has found ergot of use in cases where liquor ferri perchloridi, plugging, and other remedies had been tried without avail. The following is one of the cases he records. A weak anæmic woman, aged 55, had been suffering for three days from repeated and violent attacks of hæmorrhage from the nose, which had increased in both frequency and violence during the last twenty-four hours. The nostrils were first plugged with lint and cold water. Dr. St. George then tried plugging with lint dipped in liquor ferri perchloridi but without avail, as the hæmorrhage continued. He then ordered her a mixture, each dose to contain fifteen minims, of liquid extract of ergot every quarter of an hour until the hæmorrhage ceased, and then to be continued every four hours for a day or two. In an hour and a half the bleeding had entirely ceased and never returned. He gives the details of two other cases successfully treated by the same means. (*Ibid.*)

Treatment of Intussusception by Abdominal Section.—At the meeting of the Royal Medical and Surgical Society (Dec. 14, 1875), three cases of intussusception were read, in each of which abdominal section was performed, in two of them with success, and in the third with a fatal issue. The first case was that of an infant seven months old, who was attacked with dysenteric symptoms, sickness and griping abdominal pains. The child remained in nearly the same state for twelve days, when the pain became more severe, tenesmus violent, sickness frequent, and he grew pale and restless. The bowel projected two inches beyond the anus, and a firm cylindrical tumour could be felt in the corner of the descending colon. Insufflation and the injection of warm water failing to afford relief, the abdomen was opened by Mr. Howard Marsh to the extent of two inches, just below the umbilicus. The intussusception could not be reduced till it was drawn out of the wound, but was then readily accomplished. Some difficulty was experienced in returning the large amount of intestine that protruded. Sickness ceased at once, and a fæculent motion was passed on the third day, and the child was convalescent on the fourth. The volvulus was neither inflamed, adherent nor strangulated, and it was reduced by the application of slight force.

The second case was that of a woman aged 33, who suffered from paroxysmal pain in the abdomen for fifteen days. On the eighth day a tumour was discovered in the right iliac fossa which subsequently moved to the left iliac fossa. Inflation was tried without success three times. Mr. House opened the abdomen below the umbilicus in the middle line. The intussuscepted mass was drawn out, but reduction was only effected by a combination of pulling and kneading movements with pressure on the distal end. The length of the included bowel was at least eighteen inches. All the precautions of the antiseptic method were adopted and the patient recovered without a bad symptom.

The third case was operated on by Mr. Hutchinson; the patient was an infant six months of age. The intussusception involved the whole length of the colon, and the ilio-cæcal valve, introverted, constituted its extremity. Considerable difficulty was experienced in effecting reduction. Death occurred six hours after the reduction, the infant having previously passed a motion. Mr. Hutchinson called attention to the propriety of always seeking first the lower end of the intussusception. In the discussion which followed the reading of the papers, Dr. Buzzard remarked that from a physician's point of view 30 or 40 per cent. of cases of intussusception terminated favourably. (*Ibid.*)

The Physiological Action of Vanadium.—An interesting account of some experiments with this rare metal made by Mr. John Priestley Platt, Physiological Scholar at Owens College, Manchester, was read at a recent meeting of the Royal Society. Vanadium is nearly allied chemically to phosphorus and arsenic, but it seems to differ in many points from both of these substances in physiological action. Dilute solution of sodium vanadate was found best suited for experimental purposes and was injected into the veins, alimentary canal, and subcutaneously. However introduced, vanadium was found to be an active poison. In fatal cases, the most noteworthy symptoms were paralysis of motion, local or general convulsions, rapidly supervening drowsiness, congestion of the alimentary mucous membrane, depression of temperature, intermittent respiration, and feebleness of pulse. The heart preserved its irritability throughout, and there was no impairment of consciousness or of sensibility to pain. With regard to special functions Mr. Priestley arrived at the following conclusions:—1. That sodium vanadate acts both on the vasomotor centre and on the intracardiac nervous mechanism, producing diminution of blood pressure, disappearance of respiration curves; feebleness, irregularity, and intermittency of the pulse. 2. That it acts on the respiratory nervous centres, causing a stimulation and then a depression of the respiratory movements which sometimes become intermittent. 3. That it has no effect on nerve-trunks or their peripheral terminations, but exerts its influence on the central system of the spinal cord. 4. That it has no direct action on muscle. (*Lancet,* Jan. 15, 1876.)

Tetanic Action of Gelseminum Sempervirens.—Dr. Sidney Ringer and Dr. Murrell in a paper "On the Physiological Action of Gelseminum Sempervirens" compare the tetanus produced by its use with that from strychnia in the following terms:—1. Strychnia tetanus supervenes without previous loss of voluntary or reflex power. 2. Respiration continues unimpaired during the paroxysms. 3. Every part of the body is affected by the anterior extremities as strongly as the posterior. 4. The cord is much less rapidly exhausted; so that a fresh paroxysm could be excited directly the previous one had declined, and in this way it could be reproduced many times without any interval or apparent diminution of intensity. 5. Strychnia tetanus persists for many hours or even for several days. Gelseminum tetanus on the other hand—1. Is always preceded by considerable loss of voluntary and reflex power. 2. Respiration ceases before the onset of the convulsions. 3. The posterior extremities are most affected. 4. Irritation fails to excite another paroxysm till the lapse of some seconds, as if the exhausted cord required time to renew its energy. 5. It lasts only a short time, sometimes only half an hour, rarely more than three hours. The character of the tetanic convulsions tends to show that they are due to the action of the drug on the cord. The authors think that the plant contains two active principles, since it is difficult to conceive how a drug could first paralyse and then excite an organ. There appears to be a certain amount of antagonism between gelseminum and strychnia. (*The Lancet,* Jan. 15, 1876.)

Composition and Therapeutic Value of Dr. Warburg's Tincture.—Prof. Maclean, C.B., communicates the formula of a Tincture that has long been celebrated for its antifebrile powers, but which has been kept a secret by its compounder. There can be no doubt that it is highly efficacious, and that it has been largely used in India. It has the following very complex composition:—

♏ Aloes Socotrin. lbj.
Rad. Rhei (East Indian).
Sem. Angelicæ.

(*b*) Confect. Damocratis, āā. ʒiv
Rad. Helenii (Enulæ).
Croci Salivi.
Sem. Fænicul.
(*c*) Crit. Præparat. āā. ℥ii
Rad. Gentianæ.
Rad. Zedoariæ.
Sip. Cubebs.
Myrrh Elect.
Camphor.
(*d*) Bolet Laricis, āā. ℥j.

The above ingredients are to be digested with 500 oz. of proof spirit in a water bath for twelve hours, then expressed and ten ounces of quinine added; the mixture to be replaced in the water bath till all the quinine is dissolved. The liquid on cooling is fit for use. Prof. Maclean strongly recommends it in remittent fevers. (*Med. Times and Gazette*, Dec. 13, 1875.)

Pessary of Permanently Elastic Red India-rubber.—Mr. W. Donald Napier a few nights ago brought under the notice of the Medical Society a new form of pessary, constructed of a permanently elastic substance. It can be made of any size, is extremely smooth, and seems likely to prove serviceable. (*Med. Times and Gazette*, Dec. 25, 1875.)

Metrorrhagia Arrested by the Action of Heat Applied to the Lumbar Region.—Dr. N. Gueneau de Mussy gives several cases in which this proceeding was adopted with success. One of these was a woman æt. 26, who had always menstruated regularly, and had neither had children nor miscarriages. Abundant hæmorrhage occurred. Warm baths, astringent injections and cauterisations of the womb were employed without effect. Examination of the uterus revealed nothing affording an explanation of the metrorrhagia. In the left ovarian region there was abnormal sensibility. Sulphate of quinine and blisters to the epigastrium were now prescribed. On several occasions the solid nitrate of silver was introduced into the cavity of the neck of the womb, leaving it then to dissolve (!): the flow of blood was suspended for two or three days, but soon began again. She became gradually weaker, and suffered from neuralgia of the head and arms, with vertigo on attempting to rise. Dr. Chapman's India-rubber bag was now filled with water as hot as could be borne, and applied to the lumbar region, the application being renewed every three hours. The next day the hæmorrhage had considerably lessened, and on the following day it had completely stopped, being replaced by a slight leucorrhæa flux. The iliac and subpubic pains were increased by the heat.

Intense dyspnœa followed, which gradually diminished. At the next menstrual period the hæmorrhage recommenced, but was again arrested by the application of the hot water bag in the manner above described. From his examination of the chest in this case, Dr. de Mussy believes that it was tubercular induration, which had called into action a congestive disposition. Dr. John Chapman who translates the paper is of opinion, however, that the metrorrhagia might have been arrested by water at a much lower temperature than was really used, and attributes the increase in some of the symptoms to the mode in which the remedy was applied. He thinks a temperature of 115° F. quite sufficient. (*Med. Times and Gazette*, Dec. 11, 1875.)

Treatment of Epistaxis.—Dr. Cochrane, of Edinburgh, after referring to the method of treatment of epistaxis by ergot, recommended by Dr. George, believes that better results can be obtained by plugging the nasal passages with strips of lint soaked in a rather strong solution of liquor ferri perchloridi. Dr. Cochrane thinks the predisposing causes are the hæmorrhagic diathesis; any cause which may weaken the walls of the capillary vessels of the nasal mucous membrane; and plethora. The exciting causes are blows, catarrh, ulcerations, &c. In regard to the treatment he thinks if the case be slight, cold compresses may be applied to the nose, testis, or mammæ, or nape of neck, or ligation of the extremities, and holding the arms above the head may be tried, but he places little reliance on these in some cases, or in the internal administration of the liquid extract of ergot, aconite, digitalis, ipecacuanha, or turpentine. The only safe means that can be adopted in these cases is careful plugging with iron solution. The after treatment requires due regulation of the bowels, and the administration of such a valuable combination as the syrupus ferri phosphatis composition, in drachm doses three times daily. (*British Med. Journal*, Jan. 15, 1876.)

As an appendix to the foregoing we may add that a paper recently appeared from the pen of Dr. Englisch, of Vienna, in the *Wiener Presse* (No. 20-26), in which he strongly advocates the use of a double balloon, which can be introduced in the collapsed state, and can then be inflated. The chief difficulty is in the introduction of the bags, which are situate, one before the other behind. It can be accomplished, however, with an ordinary catheter, and a little ingenuity and patience. The balloon can be filled with water at any temperature.

Extracts from British and Foreign Journals.

Treatment of Mammillitis.—Dr. S. F. Bachelder says that there is one form of Mammillitis which does not seem to be benefited by any of the usual applications; he alludes to the condition where there is a fissure or crack at the base of the nipple, nearly separating it from the breast. Inflammation is likely to set in after a few days, and the tendency is to go on from bad to worse indefinitely. Quoting from Byford he continues: —To such an extent are the fissures of the base carried by ulceration, sometimes, as completely to amputate this projection. But there is generally at first little or no inflammation, and a careless observer would frequently fail to discover any trouble, and when found it appears like a clean cut with a sharp knife, partly around the base of the nipple. As long as it can be kept quiet it is comparatively painless, but every effort to suck on the part of the child draw the edges apart; breaking up any adhesions that may have formed, and causing agonizing pain to the mother. These are the cases where none of the plans recommended in the books ever seemed to do any good.

It occurred to Dr. Bachelder that as it was impossible to keep the edges of the fissure in apposition so as to heal, while the child nursed, they might be kept separated all the time, so that the ulceration might fill up from the bottom by granulations. Acting upon the thought, in a case of this kind, he took a strip of adhesive plaster two inches long by one quarter of an inch in width, attached one end to the nipple near the apex, and then by traction upon it drew the nipple away from the fissure, so as to open the latter as much as possible. The other end of the plaster was then attached to the breast so as so hold the nipple, in this oblique position, thus keeping the fissure permanently open. The part was then dressed with a solution of nitrate of lead in glycerine, ten grains to the ounce, and instructions were given to remove the plaster, when the babe was to be nursed, but to apply another in the same way immediately afterwards. There was no more complaint, and in two days the woman was well. (*Boston Medical and Surgical Journal*, April, 1875.)

Normal Ovariotomy.—Dr. Battey, of Georgia, has recently removed the ovaries in various cases, and Drs. Yandell and McClellan give an account of an interview they had with him for the purpose of ascertaining the reasons that led him to perform this singular operation. It appears it was first suggested to him in a case under his care, of a young woman who had no uterus, but an active menstrual molimen, whose heart was broken down by the strain upon it in the monthly vascular excitements, which were unrelieved, and of which she died. It occurred to him that if the ovaries could be removed the balance would be restored. He searched in vain for a precedent, and was afraid to make one. Since then he has performed the operation ten times. He undertakes it with a view of ridding his patient of a disease or pernicious ovulation; secondly, of availing himself of the great changes in the nervous system which attend upon the change of life and by thus revolutionising the whole female economy to enable her to throw off an otherwise incurable disease. He does not propose the operation for any case curable by other means, but considers it to be demanded in the case of any grave disease which is either dangerous to life or destructive to health and happiness. The mode in which he operates is to place the patient upon the left side semiprone, he then retracts the perinæum with an oldfashioned Sim's duckbill. The cervix uteri is now seized with a stout vulsellum and drawn down under the pubic arch. The incision is made in the median line of the posterior vaginal cul-de-sac with scissors, and from an inch and a quarter to an inch and a half in length. The incision extends at first down to the perinæum, when, if there be no bleeding, the serous membrane is opened. The speculum is now removed and the index finger passed into Douglas's fossa to work down one of the ovaries and bring it into the vagina, whilst an assistant, by pressing the hand upon the hypogastrium depresses the viscera in the pelvis. A temporary ligature is cast around the base of the ovary and the other organ similarly treated. They are both then removed by *écrasement*, about ten minutes being occupied in crushing the pedicle of each ovary, the temporary ligatures coming away with the ovaries. No suture is used in the vaginal wound, nor is any lint or drainage tube employed as a rule. When adhesions exist he uses the finger nail to dig them out. He has never met with hæmorrhage nor prolapse of intestines or omentum. He has operated ten times with two deaths, one of the latter being from febrile abscess. He believes that it does not interfere with sexual desire but that no proper menstruation occurs after it. (*The American Practitioner*, Oct. 1875.)

The Characteristics of the Action of the Heart.—Dr. Kronecker in the *Festgabe*, or festival present, given to C. Ludwig by his pupils, gives an interesting account of the action of the heart. Ludwig himself in 1866 published in conjunction with Cyon a mode by which the circulation in the isolated heart of the frog could be studied. Immediately after Dr. Bowditch showed that the heart differed from all other muscles in the circumstance that the amount of the contraction of the electrically excited ventricle is independent of the strength of the excitation, varying only with the condition of exhaustion of the muscle. If excitations succeeding to one another after equal periods no longer liberate maximum contractions of the cardiac muscle, these do not become feeble, but occur irregularly. Kronecker undertook his experiments partly with a view of corroborating Bowditch's statements. He found in the first place, that the pulse did not intermit if the minimum stimuli were applied with equal intensity, but was stopped if that intensity was slightly augmented. Warmth augments the excitability and motility of the heart; cold lowers them. At a temperature a few degrees above freezing-point the pulse may be so far lowered as only to beat once in ten seconds. The facts ascertained in relation to the trophic processes taking place in the heart of the frog are of much interest. It possesses no coronary arteries, and nutrition is effected by diffusion of the blood from the interior into the walls. The greater or less capacity of contraction of the heart depends upon its better or worse state of nutrition. The fresher and more arterial the blood, the more vigorous is its activity. If the walls of the heart remain for some time in contact with stagnant blood, the muscle loses in its contractile power, every pulse increases the energy of the succeeding one until the mixture of the used up and fresh blood has become equable throughout the vessels in connection with the heart, and *vice versâ* the beats of the heart become feebler when the blood is withdrawn. When Kronecker replaced the blood or serum contained in the cavities of the heart by indifferent solution of common salt containing about a half per cent., the pulse rapidly fell till it became imperceptible, some slight peristaltic movements could be observed, and finally the heart was arrested in diastole, and was incapable of acting even when the strongest stimuli were applied. But if now a current of blood containing oxygen were transmitted through the relaxed organ, fibrillar contractions were soon observed, then feeble contractions of the heart, and ultimately it beat as strongly as at first. These researches are of great practical importance; it appears from the facts ascertained by Kronecker that in performing the operation of transfusion it is essential that blood containing oxygen should be introduced as rapidly as possible; for the danger in great losses of blood depends not only on anomalies of blood pressure, but also on trophic disturbances of the cardiac muscle. (*Beiträge zur Anatomie und Physiologie als Festgabe Karl Ludwig gewidmet von seinen Schülern,* 1875.)

Stricture of the Urethra in the Female.—Dr. Robert Newman of New York, observes that judging from the scarcity of the literature on this subject, the conclusion would be that such a pathological condition is of a rare occurrence. No less than four cases of which he gives the full details came under his notice in the course of eighteen months. The differential diagnosis being of great importance, he thinks that in all cases endoscopic examination should be made, and he invariably employed this proceeding. In one case the patient had suffered for many years from syphilis, but constitutional treatment produced no effect. The *seats* of the stricture which were all organic were respectively at the meatus, one half inch, three quarters of an inch, and seven eighths of an inch distant from the meatus. In regard to the treatment it is claimed that electrolysis alone cured these patients. The following is the mode in which the plan was tried in one of the cases. Positive pole with sponge electrode attached grasped in the palm of the hand of the patient. To the negative pole is attached a bougie No. 12 and introduced into the urethra. This operation caused a little soreness, no doubt due to the abnormal sensitiveness of the canal. Nine cells were used during four minutes when the bougie easily, slowly but gradually passed through the stricture and entered the bladder. After the circuit was completed and stands at 0, it is gradually increased cell by cell until the current produces a feeling of warmth and a slight pricking sensation. All this time the electrode is working its way through the obstruction causing no pain. When the operation is finished the current is decreased by drawing the slide back until it stands at 0 again. A similar method was adopted in each instance. Each case had previously been submitted to appropriate treatment which had failed. When electrolysis was called upon it succeeded. It is therefore absolutely certain that the last was the only factor to which these cures can be reasonably ascribed. The patients thus treated were kept under observation and none of them suffered a relapse. (*American Journal of Med. Sci.* Oct. 1875.)

Treatment of Tuberculosis.—Dr. Steinbrück of Neuragoczi, near Halle, has tried a new plan of treatment in several hundred cases of tuberculosis, namely, placing his patients in glass rooms having a capacity of 150 cubic feet, and transmitting from 30 to 40 cubic feet of nitrogen gas (obtained abundantly from the springs of the place along with carbonic acid gas from which the nitrogen is freed by passing it through lime water) in the course of two hours. His explanations of the action of the gas is that in consequence of the deficiency of oxygen gas the patient is compelled to take deeper inspirations, as a result of which firmly adherent and deeply situated plugs of mucus are coughed up from the bronchi, and the previously blocked up segment of lung is brought into play again. The pulse falls ten or twelve beats per minute, the respiration becomes less laborious, the excited nervous system is tranquillised, and the excessive perspiration and excretion of urea and uric acid are diminished. The appetite increases together with the general strength, the capacity of the lungs and the weight of the body. A venous blood crasis is produced by inhalation of nitrogen gas which is directly inimical to tuberculosis, as may be observed in those suffering from tracheal and cardiac disease, and in pregnancy. (*Allgemeine Med. Central Zeit.*, 1875, No. 22.)

Pathology of Leukæmia.—Dr. A. Kelsch (Val de Grâce) gives an interesting account of a case of leukæmia in which he found a fresh instance of the development of lymphatic tissue in the bones. The case was that of a gendarme, æt. 28, admitted into hospital at the close of February, 1874. He had always enjoyed good health with the exception of an attack of bronchitis shortly before. There were some old cicatrices of scrofulous abscesses in the neck. Before admission he suffered from irregular febrile attacks. On examination the upper part of each thigh exhibited about twenty hæmorrhagic spots, and there were others on different parts of the body. The gums were swollen and tender. No hæmatemesis; no melæna. Tongue foul; belly hard, not painful. Liver and spleen of normal size. Severe headache. Pulse, 100; temperature high. 28th Feb.—Hæmaturia observed. 1st March.—A painful series of swollen glands made their appearance on the left side of the neck, and soon after others formed in the axillary, inguinal and crural regions. There was free epistaxis. By the 10th of March the patient was suffering from severe headache, sleeplessness, want of appetite. No albumen nor sugar in urine. On examination of the blood it was found to contain 1,000,000 red corpuscles to 14,000 white, or 71:1; the latter being consequently greatly in excess. The diagnosis of leukæmia was no longer doubtful. On the 16th March the vision became obscured, and the following diagnosis was made by M. Porcet shortly after:—" Leukæmic retinitis on both sides; retinæ of a yellowish colour and non-transparent; margins of the optic discs obscured by effusion. The retinal veins larger, darker, and more tortuous than natural; dyspnœa soon supervened with cough, and much formation of mucus in the respiratory channels. The pulse failed; vomiting occurred. There was an extensive extravasation of blood on the fore part of the head, which was followed by diarrhœa, prostration, and death. Post-mortem examination showed blood of very light colour; brain healthy; lungs with an infarctus or two of red colour. Anterior mediastinum filled with an immense greyish indurated mass surrounding the large vessels. Little worthy of note beyond ecchymoses, or accumlation of white corpuscles in the spleen, liver, and kidneys. Examination of the bones treated with picric acid, hardened in gum and alcohol and coloured with picrocarminate showed no trace of normal osseous tissue. The medullary spaces were everywhere filled with lymphatic cellules which could be easily brushed away, leaving a firm reticulum, supported by numerous capillaries filled with corpuscles. The tissue was thus converted into an adenoid tissue. The epiphyses of the long bones presented the same alteration. The hypertrophied lymphatic ganglia of the axilla only presented the characters of hypertrophy with numerous lymph corpuscles in their substance. This is only the third case that has been reported of leukæmia in which the bones have been submitted to complete examination. (*Brown-Séquard's Archives de Physiologie.* Ser. ii., t. ii., p. 492.)

The Genesis of an Epidemic Puerperal Fever.—Dr. Lusk gives an interesting account of an epidemic of puerperal fever that visited the lying-in department of the Bellevue Hospital, in New York, in 1874. During the summer months of 1873, the hospital enjoyed almost complete immunity from puerperal disorders. From 11th May to 26th August, there were 136 confinements without a single death. Then occurred a case of peritonitis from ruptured perineum which proved fatal, but did not perceptibly influence the health of the service. On the 12th and 13th September, four cases occurred in rapid succession, in all of which either the temperature rose to 103 or 104° or there was tenderness in the iliac fossæ. These cases caused sufficient uneasiness to induce Dr. Lusk to close the confinement ward. On the 21st September, a woman æt. 32, in her tenth pregnancy suddenly exhibited symptoms of collapse which proved to be the result of spontaneous rupture of the uterus. Another patient who returned and felt acutely the last scenes of this patient's death developed symptoms of peritonitis. Four other patients, however, confined on the same day, made good recoveries. In October there were three deaths from accidental complication (uræmic convulsions, puerperal mania, consumption). On the 1st November the obstetrical ward was reopened; the first patient admitted had ruptured uterus on admission, and died shortly after. 17 cases were now delivered without any mishap. The 18th died, and though her pulse had been high, the abdomen tympanitic and the temperature 106, no lesion was found at the autopsy except slight endometritis. This case exercised a very baneful influence for a long time on the obstetric department. The labour lasted 49½ hours. The first stage lasted 49h. 10m. The anterior of the cervix became very œdematous. The expulsion of the child followed five minutes after the rupture of the membranes. On the third day the temp. rose to 103⅘° P. 140 R. 34. Ten out of the succeeding 24 cases had chills. One died from pelvic peritonitis. On the 1st December, a woman æt. 28 was confined, after having a labour with a first stage of ten hours, a second stage of nearly six hours. Forceps were used. Febrile disturbance followed, with very high temp., rapid pulse, which was partially subdued by alcohol, quinine, sponging and tincture of gelseminum, but soon underwent exacerbation. Phlegmasia dolens was developed at the end of a month and death quickly followed. During the month of December from the 2nd to the 6th, seven patients were confined and all except one had rigor with more or less tenderness on the third or fourth day. One died from puerperal peritonitis. The confinement ward was again closed, and the patients received into other wards. The first effect was happy. Of 5 cases 4 had no bad symptoms. The fifth suffered from febrile disturbance but recovered. Then came a death from phthisis, and then a death in a primipara æt 36, (with long second stage of labour, causing great exhaustion) with symptoms of intense septicæmia, showing that a mischievous element had been imported into the freshly opened ward, which had only needed a sufficiently severe case of labour to assert its influence. From the 12th—19th December, there were 30 cases of confinement of which 14 had more or less febrile disturbance, and three died. In January and February, there were 50 confinements, of which 6 died, and 24 escaped without febrile symptoms. In March, 36 patients were confined and there were two deaths; diphtheritic ulcers in the genital passages were common. Great pains were taken in purifying the wards and treating the cases, but Dr. Lusk points out many difficulties with which he had to contend, and from a review of all the circumstances of the outbreak he draws the following conclusions. 1. That puerperal diseases may be engendered by the atmosphere alone. This he considers to be shown by the experience of the preceding September (1873). Here febrile disturbances were developed amongst the patients of the ward, which at once subsided on removing them to another locality, though in the new quarters no changes were made either in the doctors, the nurses, or the utensils employed. Nor was this an isolated experience. It owes its importance to the fact that the same thing had happened over and over again. The nature of the miasm can only be the subject of conjecture. The closure of the ward for the space of three or four weeks usually restored it to a healthy condition. The poisoned atmosphere was invariably generated in the ward set apart for the newly confined, and as a rule it rarely manifested any special virulence except during the winter months, when the ventilation became imperfect. At the time when puerperal fever raged with the greatest intensity, the deposit of diphtheritic membranes, characterised by colonies of micrococci upon all lesions about the genital organs seems to point to a parasitic origin. Secondly that, in distinction from the above, there is a form of puerperal fever possessing eminently contagious properties, not primarily derived from a miasm, but capable in time of generating a poisoned atmosphere. In support of this proposition it is observable that there was a patient who at a time when the general health of the ward was good, was suddenly attacked by a fever, presenting symptoms of intense severity. This patient died in a few days, and the autopsy revealed no local lesions. From the time of her attack onward there was no period when puerperal fever was absent from the obstetrical service. Removal of the patient from the ward in which she was attacked, a change in the medical staff, and the utmost precaution regarding the use of utensils were of no avail. Through the agency of the nurses the disease was kept alive and active. After the epidemic had prevailed for a season, it became characterised by the formation of diphtheritic membranes usually upon the external genitals. At first prompt cauterisation of the affected parts possessed a surprising influence in arresting symptoms that seemed to point to an inevitably fatal result. However the occurrence of symptoms preceding the completion of labour showed that the poison found other channels of entry into the economy than the lesions resulting from childbirth. At first this miasmatic condition was not sufficient to produce fatal results. Subsequently it reached a stage of development in which it alone rapidly produced death. Cauterisation of the local lesion then ceased to possess any marked efficacy. The following fact is worthy of being remembered. That three months later, the obstetrical wards were occupied by the surgical service of Dr. J. R. Wood, and that in the year's time that has since elapsed, though there have been many capital operations, not one case of septicæmia or pyæmia has occurred in that section of the "tainted" hospital. (*The American Journal of Obstetrics.* November, 1875.)

Treatment of Otorrhæa.—Dr. Laurence Turnbull of Philadelphia, in a clinical lecture on chronic suffocation of the ear occurring after scarlet fever, after giving an account of the symptoms and prognosis, observes that the first and most important matter is the removal of the secretions which may be accomplished by the syringe and a warm solution of borate and bicarbonate of soda, each one drachm to a pint of hot water. But the syringe cannot be employed in this class of cases by the patient, owing to the softened and altered condition of the membrana tympani in most instances present. If the syringe be used, it should be by the physician and with much caution. The ear douche is a much more agreeable agent, always ready for use, and not liable to get out of order. Should the patient be subject to boils in the auditory canal, wetting the parts is to be avoided and the ear should be cleansed with cotton wool. If the pus be in the middle ear and the opening in the membrana tympani small and the patient be unable to force the matter out by an attempt at violent expiration, (Valsalva's method), Politzer's plan should be adopted, which consists in blowing or injecting a little air through a tube introduced through the nostrils which are compressed by the fingers, at the moment that the patient swallows a little water. Or the Eustachian catheter may be introduced and the secretion thus blown out. In some cases simple clearing will not remove the odour, owing to the decomposition of the purulent matter and the generation of gases. This may be overcome by the use of antiseptics and disinfecting solutions, as for instance tar or chlorine water, carbolic acid, permanganate of potassium or solution of salycylic acid in the proportion of three grains to the fluid ounce, remembering that these agents must pass through the opening in the membrana tympani into the sensitive membrane of the middle ear. Next all aural surgeons are agreed upon the advisability of employing astringents, such as sulphate of zinc in the proportion of one to four grains to the ounce of water. Nitrate of silver and the salts of alum are objectionable, the latter having a tendency to cause abscess, and the former if used at all strong, facial paralysis. If paralysis should unfortunately be produced, the steady employment of electricity is necessary. According to V. Troltsch when suppuration of the ear has taken a dangerous turn lead lotions are coloured black, whilst on the other hand this colouring no longer occurs when the process tends to improvement. (*Medical and Surgical Reporter*, No. 961, 1875.)

Action of Jaborandin, or Chlorhydrate of Pilocarpine.—The alkaloid obtained from the summitates jaborandi (pilocarpus pinnatus), termed jaborandin, or better, pilocarpin, has a more uniform action than the leaves themselves, and, according to Siredey, acts somewhat differently from them. The doses recommended by Dr. Dumas in his thesis are from half a grain to two grains, from which no injurious influence need be anticipated. The action on the salivary glands is the most marked, and commenced very soon after its administration, so that in the course of five hours a rapidly increasing, and then slowly diminishing, salivation took place, especially from the submaxillary glands. The total excretion of saliva in this period amounted to 755 grammes (11,657 grains more than 1½ lb. avoir.). Copious sweating occurred one hour after the commencement of the salivation, which slowly diminished in the course of four hours. Biliary vomiting usually occurred, the urinary secretion was greatly diminished, the pulse was slowed, the temperature lowered, the pupil dilated. No therapeutic value has as yet been assigned to it. (*Centralblatt, f. d. Chirurgie,* No. 52, 1875.)

Therapeutic Value of Picrotoxine.—According to recent researches, *picrotoxine* exerts a special action on the medulla spinalis and oblongata. M. Gubler has hence been led to employ it in a case of labio-glosso-pharyngeal disease, and obtained well-marked improvement after a few days. The patient, in fact, who, on admission, was only able to take liquid food, consumed the usual diet of the hospital; the salivation had disappeared, and speech had become more easy. Picrotoxine has been administered in doses of one milligramme (·01544 grain.) At the point where the injections were made, small, hard, indolent tubercles, resembling syphilitic gummata, appeared, which slowly disappeared. This hyperplasic action of picrotoxine on the cellular tissue is, as M. Gubler observes, interesting to notice. M. Dujardin-Beaunetz has tried the effect of picrotoxine in epilepsy, beginning with smaller doses than the above, and attaining ultimately three milligrammes per diem. Here, too prompt amelioration took place in the symptoms. The fits, which in the first instance occurred every day, only took place every second day, then became more and more rare, till they ultimately disappeared altogether. The patient quitted the hospital after two months, and had not again been seen, though he promised to return should the fits recur. This result does not, of course, demonstrate that the epilepsy was cured, but simply that an attack had been staved off for two months; but it suggests that further trials should be made with it. No results have been obtained from the employment of picrotoxine in paralysis agitans. (*Gazette Médicale de Paris,* No. 51, 1875.)

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⁂ Any of the foreign works may be procured on application to Messrs. DULAU, of Soho Square, W.C.;—WILLIAMS and NORGATE, of Henrietta Street, Covent Garden, W.C.;—or BAILLIÈRE, of King William Street, Charing Cross.

ON SANITARY PRECAUTIONS AGAINST THE INFECTIOUS ERUPTIVE DISEASES.[1]

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THE group of eruptive diseases I wish to consider is more than coextensive with that of the *exanthemata;* it touches upon diphtheria and influenza, includes typhus and enteric fever, but excludes all the *enthetic* and *parasitic* diseases which are attended with distinctive skin-eruptions. Small-pox, measles, and scarlet-fever, with their nearer allies, varicella, rubeola, and roseola, will be chiefly inquired into, with the object of seeing how far our usual conduct with respect to these diseases is in accordance with the known laws of their existence and extension, as set forth by well ascertained facts in their natural history.

Precautionary measures founded on common care and common knowledge will receive an equal attention with the higher forms of social action embodied in preventive legislation and quarantine regulations; because it is only in so far as these edicts give expression both to scientific truth and popular conviction that they can be effectively carried out, and become permanently useful; while the common truths on which they are founded, and without which they could not be enforced, lie at the root of all social sanitary action, from particular cleanliness to the most wide-reaching philanthropy.

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[1] A Paper read before the last Meeting of the National Association for the Promotion of Social Science, in the Public Health Section.

Among the scientific truths that need to become common before our sanitary precautions can be made effective against the special diseases it is our duty to prevent, are first, those medical facts that serve to define the special diseases to be contended against; and second, the etiological facts which tell us how long a disease will be in showing itself in any person exposed to the risk of taking it, and how long before such person may be considered to have escaped it; or, after taking it, how long it will last, and how long it will be before the risk of giving it to others is over.

The first series of medical facts is not so abstruse or difficult as to be excluded from common knowledge; indeed it is so much part of it, that few are ignorant of the common signs of the eruptive diseases already enumerated, and except for special measures of guarding against error, few words on this part of the subject from me will suffice. On the second series of questions, if much remains unsettled, much that is perfectly well ascertained requires to be more generally known: ignorance of common facts of this kind still causes pitiable accidents in the smaller social circles, and serious mistakes in general legislation.

Among legislative bodies it would seem that, admitting infection to have occurred under certain extremes of interval in all sorts of diseases, they struck an average, and arrived at a mean which should represent an interval of safety for all; thus ten days' quarantine would seem to be settled for the Mediterranean, and I believe for many of our own colonies—some compelled to do as others do, some free to do as they like. The chief diseases quarantine rules are framed against are cholera and small-pox, the former generally showing itself in three or four days, the latter always after an interval of fourteen days. Now fifteen days for one and five for the other give twenty days, the mean of which is ten, the number of days fixed; a period quite useless for the exclusion of small-pox, and altogether unnecessary to guard against cholera. In our own ports last year, the system of inspection adopted for ships, and found sufficient for the exclusion of cholera, allowed small-pox, introduced by an emigrant ship, to be carried from one side of the kingdom to the other; that our population escaped the danger thus introduced is owing in great measure to the general protection afforded by vaccination, and partly to the efficient means adopted for isolation of the sick.

It is an important fact, as illustrating a prevalent narrowness of sanitary view, that while many ports on both sides of the world are very rigorous in the measures they adopt for the exclusion of disease as an import, they are very lax in the endeavour to prevent its export; a suspected source of disease is too often hurried away regardless of where else it may do harm, while vessels are detained for weeks on entering a port lest they should introduce a disease often already established there. This detention is quite right for certain diseases actually existing on board, or even where such have recently occurred, and the liability to infection is not over, but similar provision should be made for ships about to leave harbour under the same liability.

Of all the precautions against infection, that of isolating the sick stands foremost; it is one which can never be neglected with impunity. Not the harsher material isolation often roughly attempted by military cordons and lazarettes, but the isolation effected by a sanitary zone of fresh air, cleanliness, and care such as is needed by the sufferers themselves, and will prevent their coming in contact with the susceptible till the sickness is really over. In some diseases, such as small-pox and typhus, isolation becomes a duty.

With respect to the former, special hospitals are particularly suited, as facts on a large scale show that the disease once established is not intensified by the presence of other cases however severe, so long as the requisite accommodation for each is provided. Until quite of late we have retrograded in this matter, and the old buildings once commonly known in English towns and villages as the "pest-houses" disappeared, without being replaced by the new form of fever-wards and special hospitals. Vaccination so far suppressed small-pox, that these refuges became vacant; less accommodation of this kind is now needed, or more rarely required, but it is not safe or right to do away with this prime necessity in dealing with infectious diseases; unless the first unit of disease be controlled, we cannot be sure how far it will extend; circumstances might at any time arise which would make it spread; cold, want, war, famine, or even the temporary predominance of the social errors, which find expression in anti-sanitation societies, might admit pestilence. What this last element may do in again disclosing to us the ravages of small-pox, I will illustrate by some facts from that greatest of modern medical observers, Wunderlich, of Leipzig. In 1868, he tells us that the number of children vaccinated in Leipzig—a town then of 106,000 inhabitants, was 3,443; in 1869, the number vaccinated was reduced to 1,970, and in 1870 to 1,340. Anti-vaccination leagues, clubs, papers, had so prevailed, that one half the children born at this time were unvaccinated. One reason of this was that for twenty years small-pox was almost unknown in Leipzig. But now came the war with France; prisoners were received in Leipzig, and with the French prisoners small-pox: in little more than a year from the end of 1870, 1,027 persons died of small-pox in Leipzig, or one per cent. of the entire population; of the deaths 312 were adults, and 715 children, instead of the number of deaths being greater in those over fifteen years of age as in protected communities. Imagine such a state of things to be arrived at in London, and 30,000 children would be swept off in one year, a massacre more than Herodian, and by a form of death to which the adults who allow it, are exempted by the very means of safety which they deny to these helpless victims.

As an instance of how beneficially legislative enactment may be exercised when based on common knowledge and scientific precision, I will take the case of typhus fever in our large towns. Typhus was not only known as the scourge of camps, jails, and hospitals in the last century, but as representing, quite recently, the most prevalent and fatal kind of plague among us.

Fever suddenly spreading in any community in the early part of this century, from house to house, in a workhouse, or maternity institution, was as generally referable then to typhus, or spotted fever as it was called, as similar visitations now are to scarlet fever. It was enough for any other fever to approach typhus in frequency or severity for it to be called typhoid; or, for any other ailment to assume a serious aspect for it to be said that typhoid symptoms had set in. Now typhus fever is so rare as almost to be unknown among us, and the term Typhoid, obsolete as applied to a specific disease, is utterly without meaning as applied to symptoms. The practical benevolence of Howard; the scientific research of Priestley, who discovered oxygen in the air; the common knowledge that air was essential to respiration, and that such cubic space of it is necessary to each human being; all this combined with the special clinical work of Sir William Jenner, defining with exactness what is typhus, find their expression in the common Lodging-houses Act of Parliament, by the agency of which under my own observation in London, the hot-beds of typhus were rooted up so completely and at once, that only at rare intervals, and never for long has typhus fever reappeared in this part of England.

The way in which the Common Lodging House Act of Parliament arrested typhus, was by limiting the number of lodgers received in these houses, so that over-crowding was obviated, and by providing that any room from which a typhus patient had come should be unoccupied until after a thorough purification. All the benefit resulting from this would have been impossible, had not the medical facts distinguishing typhus from enteric fever been carefully elaborated and thoroughly mastered. It was soon seen that the infection of the one was as dependent upon impure air as that of the other has since been shown to be on impure water. Isolation and ventilation diminishes the malignancy of, and prevents altogether the one, just as cleanliness and certain sanitary measures stop the other.

Both typhus and enteric fevers, it may here be remarked, are eruptive disorders, the *maculæ* or dark mottling of the one showing four or five days after sickening, the rose spots of the other appearing still later in the illness; in both there is usually an interval of from ten to twelve days from receiving the infection before the illness results, so that care ought to be exercised for this length of time before receiving into new localities or dwellings those known to have been exposed to the infection of either. The greater intensity of infection possessed by typhus is compensated for in some degree by this disease being sooner over, while in enteric fever the infection probably lasts as long as the diarrhœa; and while instances are rare, but not unknown, of convalescents from typhus carrying infection to distant places, it is to be feared that the seeds of enteric fever are often carried to other localities, not only by convalescents but by those who, escaping themselves with slight symptoms only, yet are media for starting the complaint elsewhere. A precaution is necessary in the commencement of typhus in the young lest it should be mistaken for measles, and so chance to spread; this will receive notice at the end: and to guard against the spread of enteric fever, even while other signs of the disease may be undecided, any diarrhœa should be met by disinfectants.

At the commencement of what I have to say of the more ordinary eruptive diseases, I would draw attention to those facts by which they are distinguished from each other, and which must be borne in mind during every intelligent effort for opposing them. Until the practice of inoculation for small-pox in the earlier part of the last century no one supposed that it would take nine days to produce the disease in that way, and it was much later before the interval from infection was found to be fourteen days; indeed, before the facts collected by the late Dr. Gregory, the constancy of this interval was not at all appreciated in the manner it deserved: instead of attributing this illness to the last fancy of the sick person, we can go back definitely twelve days from the first sickening, or fourteen days from the first spots of eruption, and trace its source—no idle search, for often, gaining that, further mischief is arrested. The same thing since the time of Bateman, has been found to obtain in measles, with this difference that twelve days is more frequently the interval to the full rash than to the first sickness; this sickening, moreover, is traceable for four days before the eruption instead of for only two days, so that the real interval for measles is eight days from exposure to sickening, and this it is the more important to bear in mind, because, as is chiefly established by observations of my own, these four days are not only days of infection, but the very days on which the infection is most frequently spread. It is probable that small-pox is infectious for two days before the eruption.

The duration of infection in these two complaints is certainly not very prolonged: even in small-pox, where it is most intense, the last remnant of personal infection would seem to cease on the separation of the pustular crusts. Infection, it is known, will long persist in these; it will also cling to clothing and linger in rooms; but with baths, change of clothes, and care as to all other things which have been near the sick, three weeks would appear to be sufficient guarantee for convalescents from this disease. I have seen immunity procured in as short time after measles.

Measles has dangers of its own, which, from the favourable circumstances attending the disease here, we are apt to forget. I suppose, a fatality of one per cent. for cases of measles, even in children, would accord with our present general medical experience, and that any excessive mortality would at once attract notice to defects in sanitary conditions individual, local or general; yet we have a recent instance, nearer than Fiji, of a fatality from measles of forty per cent. among young adults. M. Léon Colin, a distinguished member of the society to which I have the honour of being secretary, informs us in his work *La Variole*, p. 151, that among the garde mobile forming part of the garrison of Paris in the month of January 1871, the mortality of measles was exactly forty per cent.: he also shows that in the Special Hospital at Bicêtre, to which he was physician-in-chief during the siege, the proportional mortality from measles during the extreme cold of that winter exceeded that from small-pox, though that disease alone in the year of the war caused more than 12,000 deaths in Paris. Beyond the individual privation and fatigue suffered by these victims to measles, the great cause of mortality was cold. The same authority tells us that small-pox was not intensified by the fall in temperature, nor was it appreciably aggravated by a certain unavoidable overcrowding in hospital, while both these causes were highly prejudicial to measles. The degree of evil from these causes may be better estimated by comparing what we already know of the outbreak of measles in Fiji with the careful record of measles in the United States army during 1863-64, preserved in the sanitary memoirs of the war. In the first year of the war there were 21,000 cases of measles, in the second year of 16,345 cases with 2,000 deaths; far short of the truth, for Dr. Bartholow (to whom we owe this valuable report) gives the proportion of deaths in two large hospitals to exceed twenty per cent. at page 219, and says this is short of the truth, for the numbers are vitiated by a false method of expressing results. Here we have happening to people of our race exactly what has lately befallen the Fijians; all recondite speculations as to inherited immunity of our own, or constitutional susceptibility of other races, vanish, and we are brought face to face with the innate dangers of the disease when circumstances allow them to appear.

From an Australian paper I quote this account by a resident in Fiji:—"The death-rate is not yet made up, but the probability is that 40,000 Fijians died during the four months' plague. The native population of Fiji is now about one-third only of what it was when I landed here twenty-five years ago. Very few died of the measles, the majority dying of subsequent disease in the form of dysentery, congestion of the lungs, &c. Want of nourishment or starvation carried off thousands. Entering a house you would find men, women, and children all lying down indiscriminately, some just attacked, some still in agony, and others dying. Some who were strong enough attempted suicide, and not always unsuccessfully. As the scourge became more permanent four or five were buried together in one grave. In some cases the dead were buried in the earthen floors of the houses, in others, just outside the house. The burials were hurried, and it is probable that some were buried alive. In many instances the husband, wife, and children all died. In one village all the women died, and in another all the men. It is said that one tribe buried alive the teacher's wife and children—whose husband and father had died of the plague—to stop infection."

In Fiji we have no crowding into hospitals, and the salubrious nature of its really warm climate has always been deservedly vaunted, yet 40,000 people, or more than one half of the inhabitants are carried off by this epidemic in four months.

Thousands were carried off by want of nourishment and care as well as by dysentery and congestion of the lungs; the worst dangers from crowding were incurred in the small houses; and the worst dangers from cold, by the people rushing at once to the water where they would continue immersed. Measles in these islands in 20° S. Lat., or within the tropics, corresponds very closely to measles in the Faroe islands in 70° N. Lat., or near the Polar circle. Intense heat seems also prejudicial to measles, for an epidemic in Australia from which that of Fiji was derived, was most fatal especially to children in the hottest part of an unusually hot summer: after the sharp fever of measles has developed the rash, a sudden depression with marked fall of body-temperature is the peculiar character of this complaint; the depressing effect of intense heat in children is well known, and consequently may become at this crisis an exceptional source of danger. Cold, overcrowding, and want, however depressing, at all times to all, are especially so at the time of this disease. Last winter, in the cold week after Christmas, a great increase in the mortality from measles was noted in my part of London, the incidence of which was chiefly on the poor. In the disease itself there was no change of type; the cold was directly injurious in some cases, but in many only indirectly. The high price of fuel aggravated the effects of cold, and instead of good fires and the free ventilation they promote, the crowded rooms, carefully closed for warmth, by affording air for respiration loaded with disease-particles conduce more to lung disease than severe cold can possibly do. It is in the early stage of measles that cold is specifically injurious; however safe and efficacious the external application of cold water may be in scarlet fever, in measles it checks the eruption on which safety depends, and abdominal or pulmonary congestion become most fatal complications. A popular appreciation of this fact is shown in the resort to the warm bath for the relief of the severe early symptoms of measles in the young: the use of the cold bath or affusion in relieving the worst onsets of scarlet fever is not so generally known; nor, that early use of the hot bath in bad cases of scarlet fever is injurious.

Scarlet fever affords a strong contrast to measles in all its characters; the interval from exposure to sickening is usually short, and the rash is always an early symptom, so that it ought not to be difficult to separate the sick from the healthy with good prospect of success; and, as a matter of experience early separation is found generally successful. Moreover there is this in favour of such a course, that if any of those removed have really received the infection, symptoms of it will appear so soon that no long uncertainty has to be provided against, and a second separation is sure to be timely.

It is well to state more formally this difference between scarlet fever and measles: on separating children from a short, definite exposure to measles, no distinctive illness is to be expected until after more than a week's interval, and they are not safe to escape for a fortnight: on separating persons exposed to the infection of scarlet fever a few days, it may be three to five, sometimes a few hours will suffice for the disease to appear: after a week of immunity, we may be satisfied that the disease has been escaped. The utter divergence from and dissimilarity of measles to scarlet fever cannot be too forcibly insisted on; these two diseases are representatives of wholly different classes of disease, measles bearing affinities with small-pox, chicken-pox, mumps; scarlet fever with plague, erysipelas, influenza, diarrhœa, perhaps also with cholera. No vestige of place for doubt remains as to the close relation of rötheln, the rash to which much attention has recently been called, to measles and not to scarlatina; so the relation of dengue a rash common in Africa and both the Indies is clearly and closely to scarlet fever. I doubt if it have any even superficial resemblance to measles. To speak of any new anomalous rash as a hybrid between measles and scarlet fever is a confusion of terms and ideas, destructive of what we know, and obstructive of further knowledge.

A popular recognition of these differences is of the utmost importance to the safe management of the sick, and especially to the safety of others. For instance, is the finely diffused rash of scarlet fever the early symptom? Then separate, if possible, others of the family: but, supposing the isolated spots or groups of spots of measles appear, doubtless the previous sneezing or coughing will not have been overlooked, and it is too late to separate others, or at least it is wrong to send them among the healthy susceptible. Again, after the rash of measles is fully out, one day suffices for the child to eat and drink and sit up; care against chill and fatigue is wanted, but the illness has suddenly declined with the throwing out of the rash. In scarlet fever the rash is only the commencement of a series of special injuries to which most organs of the body are liable for a period of three weeks, against which rest in bed for all this time is the best prophylactic; then again the special accidents of convalescence are to be feared for another three weeks. Albuminuria and the dropsy which result after scarlet fever are unknown in or after measles. Measles seldom or never recurs, scarlet fever not unfrequently happens more than once.

An important practical deduction from these facts which, if acted upon, would soon reduce the spread of scarlet fever to a minimum, and relieve our healthy sea-side towns from one of their greatest risks, is—do not seek change of air too soon for scarlet fever convalescents: they are better in their rooms for the first three weeks; they are not only safer in their homes for the next three weeks after that, but they gain strength just as fast, often faster, than if they had been sent off for change of air to the danger of the conveyances used; to the danger of the lodgings they go to; and to the danger of all with whom they come in contact. This is of special importance in the management of schools. Much may be done by domestic management in keeping convalescents not only from school, but from play,—theatres, parties, and fêtes, until reasonable fear of infection is over. School-managers have a duty to protect their schools, they should have power, so as to be able to exclude from school those known to be likely to bring infection with them. It need not be difficult to obtain a written statement of the illness a child is absent for; then let it be incumbent on the manager not to receive that child for six weeks at least from an attack of scarlet fever.

For the nearer allies of each of the diseases already treated of some variation is noticed from the typical characters just given. Chicken-pox follows small-pox so far as for the sickening to occur twelve days after exposure to infection; but, instead of the rash beginning two days afterwards, it begins at once; and some isolated spot may always be detected as soon as the first symptom of sickness is felt. There is no more trustworthy guide to so important a matter as the distinguishing off-hand whether it be chicken-pox or small-pox that may have appeared in a family or institution, than to ascertain whether the first spots were preceded by illness or not. I have known chicken-pox to be attended with so much fever, and the spots of so suspicious an aspect, that, but for this circumstance, a fear of small-pox would have remained; also, and this is of greater practical moment, I have known small-pox so masked and modified, that but for the two days of back-ache and malaise preceding the eruption, some of the necessary precautions against so insidious a danger might have been omitted.

There is the same parallel noticeable in the relation of measles and rubeola (rötheln); in the latter the eruption occurs without precedent illness, or with some faintness or catarrh of only one day's existence, instead of three or four. Perhaps as a consequence of this, certainly as a correlated fact, the period of incubation is always somewhat in excess of what occurs in measles, and may extend to three weeks. This is of importance in receiving any one who has been exposed to this form of infection, among the susceptible in any less interval. Allied to these diseases is epidemic roseola, marked by a diffused form of rash, as common during the past summer as was rubeola last year. Both are readily distinguished from the rose rashes produced by irritating matters taken into the stomach, as then the illness follows quickly upon the exciting causes be they food or physic, while rötheln, which most resembles these symptomatic rashes, occurs so long after infection that the source of it is frequently over-looked. The *roseola æstiva*, as it has been brought under my notice, resolves itself into one of these two forms of exanthem, roseola and rubeola, which have not hitherto been clearly separated. This rash differs from rubeola in aspect for it does not begin in isolated spots, but a finely diffused redness is noticeable first in the face and neck soon after slight faintness or giddiness has been felt. The small glands or kernels of the neck are palpably enlarged in both ailments, and there is soreness of the throat. This rash may be followed by desquamation which, rare in measles, is still rarer after rubeola. Roseola agrees with the scarlatina group in having an incubation of near upon a week; another strong affinity is shown to this class of disease by the occasional occurrence of albuminuria, a point contrasting strongly with the measles group in which albuminuria is not known to occur. I have met with one statement that albumen has been found in rötheln or rubeola; I suspect here the rash was of the diffused type I wish to discriminate, for I have examined for myself very many cases of rubeola without ever finding albuminuria, but in a less numerous series of this kind it has sometimes been present. The interest attaching to this consideration is very great, for seeing the close connection of rötheln with measles, roseola, as here defined, might turn out to be some derivative of scarlet fever; the more prevalent scarlet fever is the more does this prevail. Long ago the connection of roseola with summer diarrhœa was noticed by Bateman; the interchange of summer diarrhœa and winter cough is very constant and has frequently been set forth as closely correspondent. These ailments may seem slight and of no great moment to the strong, but they are serious to the weak, and are the great causes of infantile mortality, especially in great towns. It has been shown how measles has lost much of its fatality with us; could scarlet fever be mitigated and disease particles be destroyed the mortality from all the associated diseases would be lessened immeasurably. Winter cough when epidemic is intimately connected with influenza, of which the depressing effects are known to all; on the ingress of this disease in the young diffused rash over the face chest and back is by no means rare; albuminuria also sometimes occurs in this disease as it does in catarrhal croup, both of which are closely associated. Albuminuria has also lately been noted among the prodromata of cholera.

The close relationship between scarlet fever and diphtheria is a matter of common knowledge; the same precautions necessary against the one are required against the other; they agree in the circumstances that either aid their favourable progress or are hurtful to it.

The precaution necessary to prevent the unintentional spread of these diseases by sending persons among the healthy from a source of infection is that, not only should the full week requisite for scarlet fever elapse, but a period somewhat in excess of this; for while as in scarlet fever diphtheria will generally show itself, if it be taken, in three or four days, much clearer instances of it happening after a full week's separation are forthcoming than in the case of scarlet fever. One remark may come in here with reference to these severe pests, which is similar to one made on typhus, that fresh air, good sanitary circumstances, cleanliness, and isolation tend to weaken immeasurably their intensity, and that a mild form of either spreads with less fatality than the severe malignant forms generated by filth and overcrowding.

For all effectual sanitary precautions it is essential for us to set forth as distinctly as possible the fallacy of endeavouring by fixed regulations of a general uniformity to meet the requirements of diseases so diverse as these infectious forms are seen to be. The old quarantaine, or forty days of isolation, would fairly answer the purpose in scarlet fever as it did in plague, but it would be in excess of what is required for many other diseases: three weeks is a favourite time very generally adopted now for most infectious diseases: with precautions as to baths, change of clothes, &c., this has sufficed to keep both measles and small-pox out of our large metropolitan convalescent institutions. To exclude scarlet fever from these institutions the three weeks interval insisted on before admission is certainly insufficient. Nor in these days of compulsory education should children be allowed to return to school-board schools in so short a time after convalescence: a power of compulsory attendance should be compensated by a power of compulsory absence after infectious illness. In investigating the frequent cases of scarlet fever this season in my own part of London, only one outbreak was distinctly traced to a convalescent institution, one to articles of food and drink, one to sewer-gas, and by far the greater number to schools. The cases from infected food and air did not spread beyond the individuals who originally received the infection, while in the other instances not only the children at the school or institution suffered, but they carried infection to their homes with serious and fatal consequences.

To do away with infection we require first, domestic cleanliness and care, and no dishonest attempt at concealment by either doctors or parents. Then no district should be without its special hospital for infectious diseases. 1. These hospitals should be quite distinct from the cottage hospital or general infirmary. 2. They should be quite distinct from convalescent institutions, and should nurse their own convalescents until all fear of infection is over. 3. They should be supported by the public rates, for all should contribute to what is for the benefit of all. 4. They should be under stricter and more centralised supervision than is required in our voluntary establishments. 5. They should be constructed on well-considered plans, providing separate rooms for different diseases, and a probationary ward for doubtful cases.

A fair share of public attention has been shown to these requirements in London under the direction of the Asylums Board. The Hampstead agitation indicates with what dull weight prejudice clogs progress! Not only at Hampstead but in every district a hospital for infectious diseases is wanted. Such hospital should be known as a source of security and not of danger to the neighbourhood where it is situate. The London Fever Hospital has its scarlet fever ward within less than eight yards of Theberton Street, but this proximity has occasioned no case of scarlet fever in that street during all the twenty-five years the hospital has been there. When the Small-pox Hospital and Fever Hospital were close together at King's Cross, no case of small-pox ever broke out in the fever hospital, and only once did fever occur in the small-pox hospital, this was to a nurse who had steps placed to the dividing wall to visit her daughter who had fever.

Infectious disease is not the inscrutable visitation many suppose. Pestilence no longer walks in the darkness in which it was so long enshrouded. Infection is no longer a vague and pervading influence, either telluric or atmospheric; it is known to depend on particles not floating about indefinitely, but clinging to near surfaces; without care and cleanliness it may easily be carried by those who are near the sick; it cannot be carried far in the air for fresh air oxydates and destroys it, so that for the most subtle diseases the infecting distance is small. The sources of infection should in all cases be sought out; it is important to find them and I for one am never satisfied till such a quest has been tenaciously pursued; it is the only way effectively to reduce the prevalence of infectious diseases.

"The interests of all professions are identical with those of the communities within which they move and by which they live, and hence every class should seek to unite itself to others by the bonds of comprehension and sympathy, rather than to conceal its actions behind mystery and verbiage. Everything which assists the public to understand the work of medical practitioners and the difficulties by which that work is beset, and everything which diffuses a knowledge of the points in medicine about which certainty is attainable, as well as of those to which experience must feel its way through doubts, will contribute towards setting difficulties and doubts aside.

"If we were to select a subject which more than any other appears likely to throw new responsibilities upon the profession, and on which an opposition begotten of prejudice has to be overcome among the public, it would be the propriety, comparatively lately made known, of isolating the victims of contagious disease from the persons around them. Sickness in a household has been hitherto regarded as a matter of private rather than of public concern, and the only medical duty hitherto recognised in relation to it has been that of placing the sufferers under conditions conducive to their recovery. We are beginning to take a wider view, to recognise that when the sickness is contagious it may easily from a private calamity become a public one, and that it should fall under the notice of the public authorities just as if it were a fire or an inundation. Such an aspect of the matter entails new responsibilities upon the doctor, and renders him not only the attendant upon the sick, but also the protector of the healthy. He is charged with the duty of discovering and of announcing the danger, and with the duty of stating and explaining the means by which it may be averted. A small advance of opinion would enforce the performance of these duties by legal enactment, and would invest the necessary recommendations with legal authority. Among them the isolation of the sick must in many cases take precedence of all others, and the one fact which the public ought to know is that such isolation promotes the recovery of patients as much as it secures the safety of those who are still sound."—This part of a leader in the *Times* I quote in conclusion.

THE PRACTITIONER.

MARCH, 1876.

Original Communications.

ON SUNSTROKE.

BY SIR JOSEPH FAYRER, K.C.S.I., M.D.,

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SYNONYMS.—Coup de Soleil; Insolatio; Ictus solis; Erythismus tropicus; Sun-fever; Heat-fever (G. B. Wood); Thermic-fever (H. C. Wood); Heat-apoplexy; Heat-asphyxia; Calenture; Loo-marna (hot-wind stroke, Hindostan.)

DEFINITION.—Certain pathological conditions resulting from exposure to solar or artificial heat. These conditions are :—

1. Exhaustion and failure of the heart's action in syncope.

2. A condition like shock, in which from the direct action of heat on the brain and cord in exposure to the sun's rays, the nerve-centres, and especially the respiratory, are influenced, causing rapid failure of the respiration and circulation.

3. Intense pyrexia due to vaso-motor paralysis, and to the nerve-centres being over-stimulated and then exhausted by the action of heat on the body generally. Failure of respiration, of circulation, and of innervation generally result, and asphyxia follows. Recovery is frequent, but in many cases is incomplete from structural changes having taken place in the nerve-centres.

NATURE, PATHOLOGY, AND SYMPTOMS.—(*a*) Simple exhaustion and syncope may occur under great fatigue or over-exertion, or depression from any cause, during exposure to a high temperature. Or when the head and neck are exposed to an intense heat and sun-light, a condition analogous to shock may be induced, by reflex action through the vagus and vaso-motor system. There is depression of nerve force, and prostration of muscular power; the skin is pale, cold, and moist, the pulse quick and feeble. Death may occur rapidly in the state of collapse from failure of the heart. Complete recovery is frequent.

(*b*) Asphyxia and apnœa may come on very rapidly, after certain premonitory symptoms of depression and weakness, though occasionally without prodromata during exposure, *especially* of the head and spine to the direct rays of a powerful sun, when the atmosphere is much heated, and the nervous energy has been depressed by over fatigue, dissipation, or illness. The brain and nerve centres, *especially the respiratory*, are overwhelmed by the sudden elevation of temperature; respiration and circulation fail, the failure of the latter being probably due to the inhibitory influence of the vagus. When death takes place, as it does sometimes, very suddenly, during great excitement or exertion, it has been attributed to rapid anti-mortem coagulation of the cardiac myosin. This, however, though it may occur occasionally, is generally a post-mortem change, the heart's action being brought to a close by the heat; in the same manner, as it has been shown by C. Bernard L. Brunton, that the effect of high temperature on animals is first to accelerate and finally to stop the heart and especially the ventricles, in a state of contraction. Recovery is frequently complete, but sometimes tedious, and in many cases imperfect, ending in serious impairment of health or intellect, indicative of structural changes caused in the nerve-centres. The symptoms of this form of insolatio, the real *coup de soleil*, are those of sudden and violent injury to the nerve-centres—unconsciousness and cold skin, feeble pulse, all the symptoms of depression,—death resulting from rapid failure of respiration and circulation. If not fatal, reaction may result in a variety of conditions indicative of the injury done to the cerebro-spinal system.

(c) An intense state of fever, the result of the influence of heat on the nerve-centres, and through them on the vasomotor nerves, and of the heating of the body generally, by the direct action of either artificial or solar heat, may occur, quite independently of the immediate operation of the sun's rays. It comes on as frequently at night, or in the shade, as in the day or in the sunshine, especially in persons who are exhausted by fatigue, overcrowding, depression from any cause, such as dissipation, want of rest, present or recent illness, and notably when the atmosphere is impure from overcrowding or want of cubic space.

The temperature of the body rises to 108° or higher, 110°. The brain, medulla, and cord, the nerve-centres generally, and especially the respiratory, suffer from over-stimulation and then exhaustion.

Respiration and circulation fail; there is dyspnœa, hurried, gasping breathing, great restlessness, thirst, fever, frequent micturition; the skin has a pungent, burning heat. Sometimes it is dry, sometimes it is moist. The pulse varies: in some it is full and laboured; in others quick and jerking; the face and head and neck are congested to lividity, and the carotid pulsations are visible; pupils, contracted at first, may dilate widely before death; delirious convulsions and coma, frequently epileptiform in character, relaxation of the sphincter, and suppression of urine, come on, and are frequently the precursors of death, which is caused by asphyxia.

Recovery not unfrequently partially occurs, to be followed by relapse and death; or secondary consequences, the result of overheating, end in meningitis or cerebral changes which may destroy life or intellect at a later period; or permanently compromise the whole health or that of some important function. The premonitory symptoms of this form of insolatio often manifest themselves for some hours; it may be days before they culminate in the dangerous condition just described; general malaise, disordered alvine or other secretions, profuse and frequent micturition, restlessness, sleeplessness, apprehension of impending evil, hurried and shallow breathing, precordial anxiety, gasping, giddiness, headache, occasionally nausea or vomiting, thirst, anorexia, feverishness which soon amounts to a pungent heat of skin and high temperature as shown by thermometer. The surface may be dry, but it is sometimes moist, the pulse varies; and these conditions gradually become aggravated, and frequently become worst at night, when the patient passes into the condition just described, and is found unconscious or dying.

The symptoms vary considerably, but they point to a profoundly disturbed state of the cerebro-spinal nerve-centres, and to pathological changes in the organs or structures whose functions have been so gravely disturbed. Death results from asphyxia and apnœa. Recovery is often incomplete, or followed by permanent impairment of health, and generally by complete intolerance of heat and exposure to the sun.

For the first of the conditions just described, the term heat exhaustion; for the second, *coup de soleil*, or sunstroke; for the third, thermic fever or heat fever (Wood), seem the most appropriate designations; or, if all be included under one, the old term Calenture seems not unfitting.

The morbid conditions above described, being due to heat alone, are not peculiar to any country or climate, and are liable to occur wherever persons are exposed under any circumstances to great heat, whether solar or artificial. For example, soldiers marching or fighting when oppressed by weight of clothing and accoutrements are apt to suffer either from simple heat exhaustion, or that form of insolatio which results from direct action of the sun on the head and neck. This is common enough in India during the hot season; in other tropical countries, in America, and is not unknown in Europe, or even in England during the heat of summer. Soldiers, workmen, artificers, and others in factories, heated rooms, hospitals, barracks, tents, and even ships, especially in hot climates, are liable to suffer from heat exhaustion, which may pass into the dangerous condition of heat fever or insolatio.

The stokers, or men serving in the engine-rooms of steamers in the tropics—the Red Sea, for example—are not unfrequently brought on deck in a state of complete exhaustion, from which they generally recover when exposed to the reviving influence of the air, or to douches of cold water, with some stimulant if the syncope be prolonged. Soldiers or others in service when exposed to great heat may drop out of the ranks, fall suddenly, pass into a state of coma, and die immediately, or recover after being in the greatest danger, but with damaged constitutions, rendered unfit for further service or residence in a hot climate. These cases occur on exposure to the direct action of the sun's rays when the atmospheric temperature is high, and especially when unusual exertion is being made.

But the most frequent cases are those of heat fever which come on in houses, barracks, tents, ships, by night or in the day, away from the direct solar rays. A form of disease sometimes described as ardent fever, in India, is this condition supervening on the ordinary phenomena of ephemeral fever. It seems pretty well understood that heat alone is the effective cause of the so-called sunstroke. Malarious and certain hygrometric or barometric states of the atmosphere, have no special influence beyond that which they may exert on the general vigour of the constitution, or in rendering that person more or less susceptible to heat, and so far predisposing him to suffer from it.

A dry air such as that of North India, with hot winds, is much better tolerated at a high temperature, than the damp atmosphere of Bengal at a much lower one. The dry hot air favours evaporation, and thus keeps the body cool, while in the damp heavy atmosphere the natural cooling function is almost in abeyance. Vigorous, healthy persons of moderately spare frame, possessing sound viscera, and leading temperate and regular lives, in an otherwise pure atmosphere, can tolerate a great amount of heat, and are much less liable to suffer from it than those in whom these conditions do not exist. Acclimatisation has also considerable influence in conferring toleration. New arrivals are more prone to suffer than those who have become accustomed to the climate. It is well known that the native can bear an amount of sun on his shorn head, neck, and half naked body with indifference if not pleasure, that would very soon prostrate a European. But to a temperature of the air rising above a certain standard, all succumb, and natives of India suffer like others and die in numbers every year from "loo-marna" (hot wind stroke).

The exact amount and duration of toleration of a high temperature depend to a great extent, therefore, on the vigour of natural constitution and the present state of health. The natural refrigerating powers of the body when in health, are such as to enable men to support very high temperatures, much above that of the normal state of the body, 99°. Thus in the hot dry winds no inconvenience beyond discomfort is felt, so long as transpiration and perspiration is free. This cools down the body, enabling it to resist the great heat. It is obvious that in this there is a great expenditure of force, and when it fails suffering soon ensues. Disordered health, dissipation, over fatigue, anything in fact that depresses nerve power, reduces the normal physiological capacity, and consequently renders a man more liable to succumb.

The mortality from sunstroke is about 45 to 50 per cent., but of those who recover many are permanently injured, and remain invalids for the remainder of life, which is often shortened by the changes induced. These may be some weakness due to obscure structural change in the cerebrum, or to a chronic form of meningitis which affects the sufferer in various degrees of intensity; epilepsy, impairment of memory, great nervous irritability, headache, insanity, partial paraplegia, partial or complete blindness, extreme intolerance of heat, especially of the sun's heat, rendering the person utterly incapable of serving or living in a hot climate, or of enduring exposure to the sun. Or it may gradually end in complete fatuity, insanity, or epilepsy; meningitis, which accounts for the intense cephalic pain, or in a lesser degree, disordered innervation and derangement of the functions generally, thus seriously compromising the general health.

MORBID ANATOMY.—In cases where death has taken place suddenly, as from shock, there is no very remarkable appearance. The heart may be found firmly contracted, but not always so—it may be flaccid. The lungs and brain and membranes may be found somewhat congested, but not invariably. As in cases of shock, the venous trunks, especially those of the abdomen and the right side of the heart, may be found too full of blood, and the pulmonary vessels may be overloaded with blood. The blood itself is dark and gummous, and is found effused in patches of ecchymoses, and indeed rendering the body more or less livid: the coagulability of the blood is also impaired, and it is wanting in oxygen.

In death from ordinary cases of thermic fever, the lungs and pulmonary system are often deeply congested, the heart is firmly contracted from coagulation of myosin, the whole venous system is engorged, and the body even before death is marked by petechial patches, or extensive ecchymoses of a livid appearance.

The blood is generally more fluid and gummous than natural; its coagulability is impaired, and it is acid in reaction. The globules, though generally presenting no abnormal change, are somewhat crenated, and have less tendency to form rouleaux than natural, and the quantity of oxygen is much diminished. The body for some time after death retains a high temperature; when first opened the viscera feel pungently hot, and the incisions drip dark blood. Rigor mortis comes on very rapidly from early coagulation of myosin.

The brain and membranes may be found congested, and in some cases there may be evidence of meningitis. Serous effusions into the ventricles or hæmorrhage into the brain substance may be found to have occurred, and are not improbable in the congested condition sometimes existing in the head; but the disease is asphyxia, not apoplexy, and the most important changes are found in connection with the thoracic viscera.

TREATMENT.—In cases of simple exhaustion ordinary treatment is all that is needed. Removal to a cooler locality, the cold douche (but not too much prolonged), or the administration of stimulants may be beneficial. Tight or oppressive clothing should be removed, and the patient treated as in syncope from other causes.

Rest and freedom from exposure to over-exertion, fatigue, or great heat should be enjoined.

In that form of sunstroke where the person is struck down suddenly by a hot sun, the patient should be removed into the shade, and the douche of cold water being allowed to fall in a stream on the head and body, from a pump, (or as in India from the mussuck, or other similar contrivance) should be freely resorted to, the object being twofold: to reduce the temperature of the over-heated centres, and to rouse them into action. During the assault on the White House piquet in the last Burmese war, numbers of men were struck down by the direct action of the sun during the month of April. They were laid out perfectly unconscious, in their red coats and stocks (they wore them in those days, 1852), but were recovered by the cold douche freely applied by the mussuck over the head and body. In some cases rousing by flagellation with the sweeper's broom was added, and all recovered with the exception of two cases, both of which had been bled on the spot where they fell. Mustard plasters and purgative enemata may be useful.

If recovery is imperfect and followed by any indication of injury to the nerve-centres, or by the supervention of meningitis, other treatment may be necessary according to the indications. Much exposure to the sun should be carefully guarded against, and unless recovery be complete and rapid, the sufferer should be removed to a cooler climate, the most perfect rest and tranquillity of mind and body enjoined, and the greatest care be observed in regard to extreme moderation in the use of stimulants.

In the cases of thermic fever, heat being the essential cause of the disease, the object is to reduce the temperature of the body as quickly as possible, and before tissue changes have resulted from the action of heat. As the hyperpyrexia is due not only to the direct operation of heat on the nerve-centres and tissues, but to the fever set up by the disordered vaso-motor arrangements, remedies such as may influence this disturbed condition have been suggested. The results have appeared in some cases to justify the theory; and the hypodermic injection of morphia and of quinine have both been considered to produce good results by their influence on the vaso-motor nerves and their power in retarding tissue change.

Bleeding has now happily been almost abandoned. The congested livid surface, the coma and stertor which formerly suggested it are not now so treated. Bleeding has no doubt great power in reducing temperature, and there are cases in which it may still be practised with advantage; but they are, I think, the exception and not the rule. In cases where venesection has appeared first to give relief and mitigate the symptoms, the improvement has been often transient and followed by relapse into a more dangerous condition, which has terminated fatally.

I could lay down no absolute rule in this or other diseases with reference to the abstraction of blood; and it is quite possible that greater immediate danger to life may exist in an overdistended right side of the heart than in the loss of an amount of blood that might have tided the patient over that state of peril; and therefore I would suggest that each case in this respect be treated according to its own peculiar merits. The treatment generally consists in the judicious applications of cold, either by affusions or by the application of ice to the surface, the reduction of temperature being watched with a thermometer in the axilla, mouth, or rectum.

Care should be taken not to prolong the cold application too long, as danger arises from depressing the temperature below the normal standard. The bowels should be relieved, and blisters may be applied to the calvaria and neck, though I may say I have not much faith in their efficacy.

In the epileptiform convulsions that so frequently occur, the inhalation of chloroform or ether may be of benefit, but their administration must be carefully watched. The earliest and most severe symptoms having subsided, the febrile condition that follows is treated on ordinary principles—salines and aperients being given, but not to the extent of depressing the patient. The diet must be carefully regulated, and of the blandest and most nourishing nature.

As improvement progresses, other symptoms may supervene indicative of intra-cranial mischief. Where the indications are those of meningitis, the iodide of potash and counter-irritants may be used with advantage. Removal to a cooler climate is essential: as a general rule, it is desirable that the sufferer should not, for a long period at least, return to a hot or tropical climate, and he should be guarded against all undue exposure to heat, work, or mental anxiety of any kind.

The sequelæ of sunstroke are frequently from such causes most distressing, and render the patient a source of anxiety and suffering to himself and to his friends.

The less severe symptoms—those, probably, indicative of the slighter forms of meningitis—or of abnormal brain or nerve change—occasionally pass away after protracted residence in a cold climate, but they are not unfrequently also the cause not only of much suffering but of shortening of life. It is not possible in a short notice to describe all the conditions that may result. They point to permanently disturbed, if not structurally injured, cerebro-spinal centres, and the treatment required is as varied as the symptoms presented.

EXTREME SLOWNESS, FOLLOWED BY GREAT IRREGULARITY, OF THE PULSE, WITH NO ORGANIC DISEASE OF HEART, TREATED BY BROMIDE OF POTASSIUM AND BELLADONNA.

BY R. SOMERVILLE, M.D.

ATTENTION having been called some little time ago in the *British Medical Journal* to cases of extreme rapidity of pulse, I am reminded of a case of extreme slowness, which ought to have been published before this time.

A professional man, aged 37, temperate in his habits, not a smoker, who had enjoyed good, but not robust, health, had for some months been subjected to a great deal of worry in business. For a few days before I saw him he had been out of sorts from irritation of the stomach, but on the night previous to his pulse becoming so slow, he had gone to bed feeling much better, and in hopes of being able to resume his ordinary duties in the morning. He had most imprudently, however, eaten salt fish to supper. He spent a restless night in consequence, and towards morning vomited several times. I saw him at 9 A.M. By that time he was up and dressed. He made great complaint of a feeling of oppression at the epigastrium. His tongue was clean. His pulse was normal in all respects but in that of rate. Its beats numbered only *twenty-five* in the minute. He was sent back to bed and was ordered a glass of brandy, and to have a mustard poultice applied over the stomach.

11 A.M.—Patient no worse, but there is no improvement. To go on with the brandy, taking also beef tea, and some light farinaceous food.

10 P.M.—In all respects the same. There has been no vomiting during the day. The rate of the pulse continues the same: it does not count more than twenty-five. The action of the heart, except as regards rate, is normal.

Second day.—Pulse has, during the night, quite changed in character: it is much quicker, and very irregular. The action of the heart is tumultuous. No murmurs to be heard. Patient still complains of great oppression at the præcordium, but there is no vomiting. He is very desponding as to his prospect of recovery. Was seen to-day by Dr. Begbie of Edinburgh, who was of opinion that the disorder of the heart was functional only, and who advised large doses of bromide of potassium with tincture of belladonna.

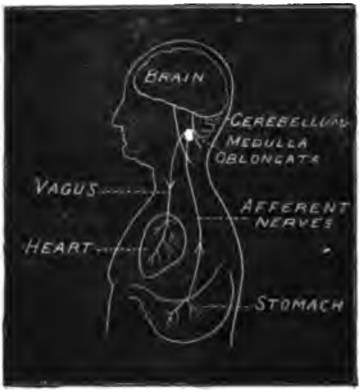
Third day.—Pulse and heart as yesterday, but præcordial oppression scarcely so great, and patient rather more hopeful.

Fourth day.—Still great irregularity of the pulse, and heart very nearly as tumultuous in action. No vomiting. Bowels acting normally.

Eighth day.—The heart and pulse have, since last entry, continued to improve, and to-day they have attained to perfect regularity of action. Patient is gaining strength, being now able to take substantial nourishment.

It is now more than three years since the above notes were taken, and the gentleman whose case they detail has since then enjoyed good health, and has remained perfectly free from all cardiac symptoms.

[There can be little or no doubt that the peculiar symptoms observed in this case were due to irritation of the pneumogastric nerve. It was long ago discovered by Edward Weber that this nerve, when irritated, either stopped the heart altogether or greatly lessened the frequency of its pulsations in animals, and in 1865, Czermak showed that it possessed the same power in man. This physiologist was able to irritate his own vagus nerve on the right side by pressing with the finger at the inner side of the sterno-mastoid muscle, and thus squeezing the nerve against a small, hard lump, which I have heard, from good authority, was really an exostosis from one of the cervical vertebræ, although it is often said to have been an enlarged and hardened gland. Whenever he did this, his pulse fell to less than half its normal rate, and he experienced a peculiar sensation of oppression or constriction in the sternal region. As these were exactly the symptoms presented by the case just narrated, I think we may fairly conclude that both the slow pulse and the epigastric oppression observed in it were caused by irritation of the vagus, and may proceed to consider what part of the nerve was affected. The nerve may be divided into three parts, viz.: its roots in the medulla oblongata, its trunk, and its terminal branches in the heart. The effect upon the pulse will be much the same whatever part of the nerve is irritated. The medulla is the part in which alterations in the activity of the nerve, leading to changes in the pulse rate, usually take place. The trunk is only affected in rare instances, such as that of Czermak; but the terminal branches in the heart are more frequently acted upon by poisons such as nicotine, conveyed to them by the blood. These poisons may at the same time exercise a similar action on the medulla. In the present instance there was nothing whatever to indicate that the trunk of the vagus was affected, and the point of irritation might be either the roots in the medulla or the ends in the heart. Now, the supper of salt fish was evidently the cause of the disturbance, but it is not very easy to say whether it produced its effects by acting as a poison or simply by irritating the stomach. There are a good many instances on record of a subtle animal poison becoming



developed in salt fish or sausages; a poison which not only eludes the efforts of the chemist to detect it, but which seems to have a curiously limited action, frequently doing no harm to the lower animals, though proving fatal to man.[1] Although the symptoms observed in this instance differed considerably from those usually produced by the poison generated in salt fish, it is nevertheless possible that this was a case of the sort, and that the peculiarities it presented were due to the poison being in very small quantity, but acting on a subject rendered exceedingly susceptible by previous overwork. If present at all, the poison must have been in such small quantity as not to affect persons in ordinary health, for the others who partook of the salt fish at the same time experienced no inconvenience from it. But it is also possible that the fish produced slowness of the heart simply by irritating the stomach, and thus causing reflex irritation of the vagus and slowing of the cardiac pulsations, as Meyer and Pribram found pinching the stomach to do.[2] If such were the case, the irritation would be conducted by the afferent nerves from the stomach to the medulla, and thence downwards by the vagus to the heart, as represented in the diagram.

It is, as I have already said, exceedingly difficult to decide whether the salt fish acted in one or other, or both, of the ways just mentioned. In most cases of poisoning by salt fish, the pulse is either normal or quicker than usual, and this militates against the supposition that the slowness in the present instance was due to a special poison generated in the fish; and, on the other hand, although the slowness continued a considerable time after vomiting had ceased, there was probably still some gastric disturbance present, as it was not until the eighth day, when the pulse had become almost quite regular, that the patient was able to take substantial nourishment. It seems, therefore, not improbable that the affection of the heart was due to reflected irritation from the stomach, and, although the symptoms were so different, might be classed along with those cases in which we find increased pulsation and palpitation depending on gastric disorder.

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[1] Husemann, *Handbuch der Toxicologie*, p. 331.

[2] Meyor and Pribram, *Wien. akad. Sitzungsber*, July, 1872.

It is, however, also possible that a special poison might have something to do with it, and the treatment adopted was such as to meet both possibilities.

Supposing it to be of a reflex nature, the bromide of potassium would be useful, as it seems to have the property of diminishing the activity of reflex centres generally, and by acting in this way upon the medulla, it would prevent the irritation it had received from the stomach from being transmitted down the vagus to the heart.

If, however, there was a special poison in the blood, irritating either the roots of the vagus in the medulla or its ends in the heart, the bromide would be of little use, but belladonna would be successful. For belladonna and its alkaloid, atropia, in large doses, paralyse those nervous structures of the heart in which the vagus ends, so that no irritation of any part of the nerve whatever can slow the pulsations. As doses of belladonna, large enough to paralyse the vagus, usually produce dryness of the mouth, delirium, and other unpleasant symptoms, the drug was not pushed to its full extent in the present instance, trust being rather placed in the bromide; although, had the interference with the heart's action been such as to threaten danger to life, recourse might have been had with advantage to large doses of belladonna.—Ed. *Pract.*]

ON SYPHILIS.[1]

BY DR. v. SIGMUND,

Professor of [redacted] *Syphilis in the University of Vienna.*

In this paper I have confined myself to the results of the clinical observation of facts during a tolerably long career spent in constant attendance on the Vienna General Hospital, and have not attempted to enter into histological questions.

Syphilis is a disease completely distinct and separate from all forms of gonorrhœa; on this point all observers are agreed. But there is a disease which is most frequently, and indeed almost exclusively, propagated by sexual intercourse, which is characterised by the appearance of an ulcer, usually, though not always, seated on the surface or mucous membrane, especially of the generative organs, or their immediate vicinity, and which, like gonorrhœa, is a *purely local* affection. In many cases, coincidently with the formation of pus in the ulcer, and sometimes with its confinement and burrowing in the connective tissue and skin, *inflammation arises in the lymphatic vessels and glands,* but in all instances it does so only in the neighbourhood of the ulcer, and always *in direct anatomical relation and connection with the seat of the ulcer*—never without this, never isolated or at a distance from it.

This inflammation runs its course *without* in any instance causing general disease of the organism. Thousands of cases have fallen under my notice in which I have been able to satisfy myself on this point by observations continued for years. Moreover,

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[1] A paper by Professor Sigmund, on the Treatment of Syphilis, which has been delayed by the illness of his colleague, Professor Hebra, will appear in an early number of the *Practitioner.*—Ed. *Pract.*

I have seen cases of repeated infection, and in one of our ablest soldiers, who died a hale old man at the age of 83, without offering *a trace* of constitutional syphilis, the infection was repeated as many as twenty-four times. Such ulcers constitute, it is well known, the subject of dispute between the so-called dualists and unicists. Nothing that has been adduced on either side is capable of absolutely deciding this question: clinicists must however maintain the dualism of the disease, until better grounds than those at present advanced for unicism have been discovered. The so-called venereal ulcers furnish inoculable pus which even in syphilitic patients always reproduces syphilis in all stages, phases, and forms, and these ulcers make their appearance in individuals who have never suffered from syphilis, just as upon those who have long suffered from it and now appear to be healthy. Undoubtedly under the general term ulcers, different forms of disease are included, as diphtheritic ulcers, contagious venereal ulcers, follicular ulcers, &c.; nor can there be any doubt that loss of substance of the skin, in all points similar to the so-called soft chancres, can be produced by the inoculation of simple pus. Precise histological as well as chemical and clinical researches will alone establish the fact of what I have just stated. All the attempts of the unicists to view these ulcers as different grades of a true venereal sore depending on the greater or less intensity of the syphilitic virus, are untenable on an unbiased examination. The virus of syphilis has further the peculiarity that it is not simply communicated by contagion, but that it infects, *non contactu sed infectione sanguinis et nutritionis totius organismi agit.* And a poisoning of the blood and consecutively of the nutrition of the body at large, syphilis is, and remains, from its first germination at the point of contagion to its passage into the lymphatics, and from thence into the blood vascular system, until finally it passes from the defined forms which it assumes in various systems and organs, into the ultimate syphilis-cachexia. That the germ of syphilis does not produce in every one who may be affected, *all* the well-known series of symptoms indicative of such morbid phenomena is a remarkable fact; but this is explicable if the individual attacks are studied carefully and for a sufficient length of time. For example, I have directed the attention of my pupils for years to the comparatively mild course of syphilis in the menstruating woman, but I have demonstrated many other causes of such a mild course, which all point to the fact that its course is simpler or milder when we strive to preserve the several physiological functions as far as possible in a healthy state, to remove all causes of disturbance, and moderately to increase the secretions and excretions, whilst as far as possible we prescribe a complete system of hygiene and dietetics, along with mercury or the iodides. That the *former* in the earlier stage (up to the papulür eruption) is only rarely and exceptionally applied as a general remedy has often enough been seen in my clinical practice.

Now there is still another point, viz., the inoculability of syphilis. If we were going to speak of experiments on this point, the mere methods of performing them would require special attention and criticism, and observations of their results and conclusions founded upon them would do so still more. But we will pass them over and consider accidental inoculations, in regard to which the following facts have occurred to me. So-called primary and secondary syphilis is inoculable: upon this point all are agreed. The gumma too, and, I repeat, the substance of the gumma as well as the blood taken from the gumma, is also inoculable. This is associated with the fact that gummous mothers bear children which do not at first appear syphilitic; but often, no doubt, distinct though perhaps not glaringly palpable evidences of the disease are overlooked during the first years of life. These children subsequently present unquestionable gummatous forms of syphilis. Let not the dogmatists be too hastily misled by the still more hasty critics. I venture to give this warning to those who believe the question to be decided as to whether father or mother give congenital syphilis. I have never succeeded in recognising true syphilis in a child in which the uterus that bore it was not also syphilitic. But unfortunately I know many, many cases in which the uterus, on *scrupulous examination and protracted observation*, was found to be diseased in women considered to be healthy so far as syphilis was concerned, and where the syphilis in the child had been attributed to the semen of the father.

When trouble and time were taken to investigate carefully and for sufficiently long periods all the organs and systems of the body, it was found that besides general syphilis of the lymphatic glands, very frequently obstinate necrobiosis of the epithelium and of the corium existed in the cervix uteri, in the tonsils, the pharynx, &c., as well as extremely slight annular syphilides of the skin, very delicately marked psoriasis, &c. Time forbids me to enter further in this paper on the subject of the local and general disturbances of the blood-forming organs, and of the nutrition of the body, which occur in such persons.

But the whole chapter of this so-called latent syphilis (*latet illum qui non noscit*) is of the utmost interest to every practitioner who does not consider his whole duty to consist in paying attention merely to one form of the disease or to a few short moments in the life of the individual.

When more than twenty-five years ago (in complete accordance with my honoured guide Virchow) I declared syphilis to be a disease essentially affecting the whole lymphatic system, I had carefully studied the question of the extension of syphilis germs in not a few individuals. In perfect accordance with Virchow I have always regarded syphilis in all its stages as a disease which has its origin in new cell-growth, which cell-growth is incapable of becoming vitalised and soon passes into organic misformations and degeneration of various kinds. On this account I have also called it "trophic disease." It is for this reason also that in my clinical work I constantly inspect and examine the *whole* body. Nothing is more prejudicial to a thorough pathology of syphilis than to regard it as a skin disease, because many things are more readily observed on the skin than elsewhere *in primo introitu*. *Lenitius novit qui penitius pervestigavit.* Even at that time I directed the attention of the profession to this characteristic mode of extension *as the most constant type of the disease.* Look at the most recent histological researches, and if you inquire without prejudice you will see that from the simplest cell-infiltration to the gumma, according to the mode of the origin and extension it will always be found that first the lymphatic vessels, then the most delicate capillaries of the blood-vascular system, and ultimately the lymph and blood themselves, form the carriers of disease. This fact affords as complete an explanation even of the gummatous disease of the vessels in syphilis of the brain as it does of the origin of these minute gummatous nodules and nodes in the male organs of generation, which often become very hard in chronic cases of syphilis, and are then regarded as cases of reinfection and of new syphilitic infiltration of a foreign nature.

We have recently seen hard nodules of this description in the labia majora of a woman, who also presented similar gummata of cartilaginous hardness upon the body.

Now it is well to discuss the subject of syphilis, since science and humanity can only benefit from such discussion. Let all personal motives be thrown aside; one thing comes out clearly from these discussions. Public instruction has still a very important duty to fulfil in regard to medical science. Special classes for the clinical study of syphilis ought to be formed everywhere, and practitioners should everywhere be instructed in them in such a manner that they should be made to diagnose, even if they are not always made to treat, all forms of the disease. I need hardly observe that thereby the most effectual barrier would be erected against the ignorance, arrogance, and charlatanry that prevail in this much dishonoured region of scientific and social hygiene.

ON THE TREATMENT OF SUNSTROKE BY THE SUBCUTANEOUS ADMINISTRATION OF QUININE.

BY SURGEON A. R. HALL,
Army Medical Department.

THE experience of several medical officers in India is now apparently sufficient to prove that the hypodermic injection of quinine in heat apoplexy is the most successful method of treatment that has yet been adopted. I have had conversations with several who have used it; and a friend of mine, a Surgeon-major recently returned home, said to me:—"If there is anything in the practice of medicine which may be described as *magical*, it is the effect of the subcutaneous injection of quinine in sunstroke." Records of a good many recoveries have been published; but as a typical case, I select the following one recorded by Surgeon-Major T. C. O'Leary, M.B., Royal Horse Artillery, in the "Annual Medical Report of the Army for the year 1872." As I presume most of the readers of the *Practitioner* do not see this blue-book, I insert the case here in full:—

"A man of the D Brigade, Royal Horse Artillery, was, with others, unavoidably exposed in shifting baggage at the break formed by the Kistna river, on the line between the Madras and Bombay Presidencies. He was young, healthy, and had not been drinking. The train was about to start, at three o'clock in the afternoon, from Raichare, when he staggered up to the carriage set apart for the sick, and asked for admission, saying 'he was dying.' The bell had rung, and no time was to be lost. The water chatties were filled, a lump of ice procured, and the two medical officers accompanying the Brigade entered the carriage with him. The cold douche was assiduously employed, ice was applied at the nape, and friction of the limbs kept up. Though the temperature of the surface was brought to its natural standard, the patient was making no rally, and the heart's action was rapidly failing. Stimulants could with much difficulty be administered, as the jaws were firmly closed, and complete insensibility was almost established; in short, the case was becoming desperate.

"Quinine had been recently recommended in cases of this nature, and, as a syringe was at hand, it was determined to introduce the drug subcutaneously. The effect was closely watched, and after the first injection the pulse at the wrist was felt to flicker; this encouraged a further trial, and the result was so satisfactory that a third injection was accomplished. Within a few minutes the circulation was fully established, the man sat up, stared vacantly about, and recognized those standing near.

"It is only necessary to add, that within half an hour from the time when the first injection took place the patient was partaking of mutton broth, and was partly feeding himself. He soon fell into so comfortable a sleep that the medical officers were at liberty to leave him, and on the arrival of the train, at seven o'clock in the evening, at Goolburga, he was practically convalescent. On reaching Kirkee next morning he jumped out of the hospital carriage, and would have marched to barracks had he been permitted.

"The jolting of the carriage did not permit a nice calculation to be made of the quantity of quinine introduced at each injection, but the apothecary was of opinion that five grains, in all, must have been passed under the skin. No ulceration took place at the points of entrance of the needle."

During the month of May 1869 my friend, Mr. Walter Kerr Waller, of Calcutta, told me that he had been very successful in treating sunstroke by doses of 20 or 30 grains of quinine given by the stomach, and advised me to try it. A short time afterwards, I was called to see a driver of my battery, at Barrackpore, near Calcutta, who was a patient in hospital, and who was knocked down with heat apoplexy about five o'clock in the afternoon. I found him completely comatose, with dilated pupils, stertorous breathing, face very much flushed, skin of body red and *burning hot*, pulse full and rapid. I dissolved 20 grains of quinine in 20 minims of dilute sulphuric acid, and about 3 ozs. of water, and attempted to make him swallow it; but in vain. I therefore thought it a good opportunity for trying the hypodermic method. A solution of 5 grains of quinine in 5 minims of dilute sulphuric acid, and

50 minims of water, was put under his skin, in different places, about the shoulders. Within one hour, the heat of surface had perceptibly decreased; he steadily improved during the night, was quite sensible next morning, and recovered without any bad symptoms. As far as I am aware, this was the first case in which quinine was *hypodermically* employed. Surgeon J. Anderson, at present with the "Chestnut troop" of Royal Horse Artillery, shortly afterwards, in the same hospital, treated a case with equally satisfactory results. I attended five cases of heat apoplexy at Barrackpore, and employed this method, and they all recovered.

Now, as to the condition of the patient, and the way in which the remedy acts. Heat, at first, acts as a stimulant on the vaso-motor centres, causing the heart to beat more forcibly and rapidly. But after a long time, the over-stimulated centres become exhausted; then the capillary vessels are dilated fully. This condition is now generally recognised as one of real debility. A writer in the *Lancet* of February 3, 1872, under the head of "Therapeutic Traditions," remarks:—"For the old idea, that sensible heat of skin with redness of the face in itself implies strength of constitution, no authority remains; the obvious fact being that surface redness means *vaso-motor paralysis.*" One prominent symptom is noticed in heat apoplexy; that with increased amount of blood in the skin, there is entire suppression of *perspiration.* The sudoriparous glands have apparently lost their power of action. I have an idea that the pathological conditions of heat apoplexy, and the *secondary fever* of cholera are very like one another, each a state of exhaustion, the consequence of previous stimulation, and that in both these states stimulants and quinine do good.

The action of quinine when introduced into the system has been studied experimentally by Binz, Briquet, Manassein, Chaperon, Eulenburg, Setschenow, Schwalbe, and several others; and many of these observers are of opinion that this drug, in large *doses* (30 to 90 grains,) acts as a sedative to the circulation. Dr. H. C. Wood, in his "Treatise on Therapeutics," writes, p. 58:—"I have never, however, been able to perceive any such action in man after ordinary therapeutic doses, and have met with no experiments on animals indicative of its occurrence. I believe, therefore, that in tonic doses, quinia produces no perceptible sedation of the circulation." Accurate observations with the sphygmograph and other instruments, of the effects of this remedy on the nerve centres and blood-vessels seem to be desired. Do not moderate doses stimulate both the vaso-motor *and* the inhibitory centres ? and as the effect on one or the other preponderates, so there may be quickening or slowing of the heart's action ? Dilated capillaries in sunstroke may be produced by stimulation of the inhibitory centres and depressor nerve of the heart, or by exhaustion of the vaso-motor centres. I think that observations favour the latter view. Quinine may reduce the temperature of the skin by exalting the action of the inhibitory apparatus, and allowing the vaso-motor centres to produce contraction of the blood-vessels; or may act as a direct stimulant on the vaso-motor centres themselves. I have not space to enter into the subject here; but a consideration of these questions will, I hope, form the subject of a future paper.

ON SALICYLIC ACID AS AN ANTIPYRETIC.

BY DR. C. A. EWALD,

Assistant Physician to Prof. Frerich's Wards in the Charité Hospital, Berlin.

It is only a few months since that Dr. Butt, from Basle, published a short paper on the effect of the internal administration of salicylic acid (*Centralblatt für die Medic. Wissenschaften*, 1875, No. 18) in reducing the temperature of the body, and to-day we have in our German literature no less than eight or nine authors writing on the same subject. There is ample evidence that some of these authors have committed the mistake of coming to premature conclusions from insufficient experience. But the greatest, and at the same time most trustworthy, portion of these observers has pronounced on the whole such a unanimous verdict upon the effects of salicylic acid that we can come to definite conclusions as to its value.

Since last summer I have myself made use of salicylic acid, and its salts, in Professor Frerich's wards in the Charité Hospital, and I communicated my observations to the Berlin Clinical Society at a meeting held some time ago. I believe myself to have arrived at an independent estimate of the action and the value of this drug; and since the subject seems to have received but little public attention at the hands of the profession in England, I gladly avail myself of the opportunity of publishing the results of my observations in the pages of the *Practitioner*.

My experience extends over upwards of one hundred cases, and the administration of from three hundred to three hundred and fifty single doses. The majority of these cases were cases of typhoid fever. The remainder included nearly all other acute diseases accompanied by high fever, and also cases of phthisis. The general action of the drug was the same in every case, the only difference being one of intensity. The administration of salicylic acid and its mode of operation must therefore be the same in every case, where reduction of temperature is the end desired. I will now consider the several points in detail.

1. *Preparation and Dose.*—It is a matter of indifference whether the pure acid or one of its salts, best the sodium salt, be used. As, however, impure preparations, containing carbolic acid, &c., have a strong astringent and even caustic action, and since, in all probability, the acid undergoes conversion into the sodium salt in the blood, it is better to use the sodium salts at once. Whether however, the acid or its salt be employed, it is necessary that each dose should be a large one, as small or divided doses have little or no effect. Generally speaking, the minimum dose required to reduce the temperature effectually is 5·0 grammes (77 grains). Should such a dose have no effect, one may after four or five hours with safety give a second, or indeed even a third similar dose. I have seen cases in which 15 or 20 grammes of the salt have been given during twenty-four hours without any ill effect. The salt is readily soluble in water, and is taken by most patients without complaint. In those rare cases in which malaise or vomiting occurred, three or four drops of chloroform sufficed to remove them.

2. *Action on the Temperature.*—As a matter of course, the effect of the salicylic acid was not the same in each case, but it has proved to be more trustworthy and efficacious in its action than all other antipyretics. I have collected the results of one hundred single doses in cases of typhoid of nearly equal severity. In order to test the value of the drug more accurately, it was always given at midday, before the usual afternoon rise of temperature. Almost immediately, the temperature began to fall, the maximum result being reached, in most cases, from four to five hours after the administration of the medicine, but in some cases not less than eighteen to twenty hours were required. The subsequent rise of temperature was as gradual as its fall, but in mild cases it never reached the same height as before. These facts were established in many cases by thermometric observations made every ten minutes in the axilla and rectum. The following are the results obtained in this way, which have been confirmed by further experience. Where the drug had an effect the maximum fall, after its administration, was 4·3° C., where it had none, the maximum rise was 1·5° C. The temperature at 5 o'clock on those days on which, at 12 o'clock, 5

grammes of salicylic acid had been given, was more than one degree lower than the temperature at 8 o'clock in the morning of the same day, in 45 per cent., and less than one degree in 35 per cent. of the cases. On the whole, then, a reduction in temperature was observed in 80 per cent. The temperature was increased in 20 per cent., and in four cases the increase exceeded 1° C. These results prove the superiority of salicylic acid over all other known antipyretics. The accompanying curves show the effect of salicylic acid in two cases.

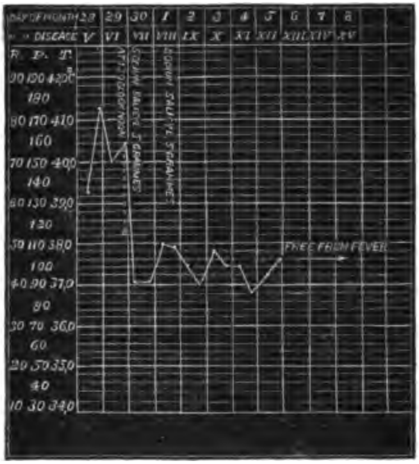


FIG. 1.—Patient aged 33. Typhoid fever. Spleen palpable, delirium, roseola up to the twelfth day of the disease.

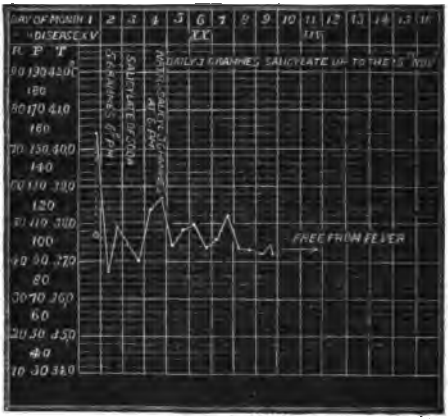


FIG. 2.—Patient aged 30. Typhoid fever. Spleen still distinctly palpable on the twenty-fifth day of the disease.

3. *General Action.*—Within fifteen minutes, or even less, after the administration, a copious perspiration breaks out, first on the face, then on the thorax, abdomen, and the rest of the body, accompanied by reddening of the skin, more especially of that of the face, and may be so copious that the patients may lose 500-750 grammes of water. Almost simultaneously with the outbreak of this sweating, sometimes a little later, the temperature begins to decline, the gradual fall lasting much longer than the perspiration. Nor is there, indeed, any constant relation between the fall of temperature and the amount of sweating, there being in many cases great reduction of temperature with little or no sweating. Generally the pulse and respiration are not at all affected, though the pulse may become a little slower. Where the pure acid or the sodium salt are employed, the intestinal tract does not appear to be at all affected. The recorded accounts of irritant effects on the mucous membrane of the œsophagus, stomach or intestines, even of erosions and hæmorrhages, are due to admixture of irritant substances, such as carbolic acid, with the salicylic acid. This is shown by a comparison between my earlier experience with my later, as I have not met with any such results since using the pure drug. The evacuations, on the other hand, become more frequent and fluid. The salicylic acid appears in the urine, which is otherwise unaltered, as salicyluric acid. The cerebral functions appear little or not at all interfered with, for so far as my own experience goes, only three patients have complained of buzzing in the ears and dizziness, and only one of hallucinations. Nor does such a collapse occur as one might have expected from the great fall of temperature. I have not, nor indeed have any others, ever seen a fatal case of collapse, and although several patients, especially such as have sweated profusely, appear during the fall of the temperature or shortly afterwards much exhausted and very pale, this condition is at most rare and transitory. As it is quite possible that owing to so great a reduction of the heat of the body, a fatal collapse may occur, I have not been in the habit of giving the acid to very debilitated patients, or when I have done so I have at the same time administered analeptic and stimulating remedies.

4. *Physiological Action.*—Of this, strange to say, we know

nothing. One thing only is certain, that Dr. Butt was wrong in his opinion that the similarity between the action of salicylic acid and other antiseptic remedies upon the temperature was due to its antiseptic properties, for the sodium salt, which has little or no antiseptic properties, is identical with the acid in its effect upon the temperature. The question clearly is, whether the process of oxidation is diminished throughout the whole body, or whether only those parts in which we make our thermometric observations are reduced in temperature owing to altered thermic conditions. This can only be established by comparative measurements of the heat of internal and external parts of the body. I am engaged in comparing the temperature of the stomach and peripheral parts by means of a thermo-electrical apparatus, and hope to arrive at interesting conclusions.

4. *Action in different Diseases.*—In most diseases accompanied by acute or chronic fever, it is certain that salicylic acid has no effect upon the local process. For example, in pneumonia, erysipelas, acute exanthemata, phthisis, pleurisy, &c., where the temperature has been reduced by the action of the drug, we see no change in the local processes. All authors indeed are unanimous in asserting its uselessness in ague. In two diseases only, typhoid fever and acute rheumatism, has no definite conclusion been arrived at, though Dr. Riess, whose experience is so far certainly the largest—more than 400 cases (*Berl. klinische Wochenschrift*, 1875, No. 50)—is of opinion that by the use of salicylic acid the course of typhoid fever is shortened. Dr. Goltdammer (*Berl. klin. Wochenschrift*, 1876, No. 4) and myself, however, whose material is second only to that of Dr. Riess, regard this question as not yet settled. Indeed we all agree that the mortality this year has been certainly no less, and perhaps even greater than in former epidemics of typhoid, while on the other hand, we are also unanimous in the observation, that under the daily treatment by salicylic acid, cases which entered the hospital during the first or second week of the disease with the temperature from 40-41° C, became free from fever after the second or third day, although the local processes (tumour of the spleen, roseola, diarrhœa, &c.) continued. It is far easier to arrive at a definite conclusion regarding the influence of the acid in acute rheumatism than in typhoid. Indeed, one may say with

certainty that in *many cases* after three or four doses, or even after five or ten grammes, not only is the fever reduced but the *articular pains* also are dispersed, so that *in a few days acute cases may be looked upon as cured.* Whether, however, the tendency to relapses and inflammation of serous membranes is lessened is doubtful, and indeed from my own experiences must be negatived.

It will, I trust, be clear from these remarks, that we have not to do with a drug of ephemeral notoriety, such as Jaborandi and others, but with one which must take a permanent place in our pharmacopœia.

NOTE ON A NEW METHOD OF ADMINISTERING ETHER VAPOUR.

BY LAWSON TAIT, F.R.C.S.,

Surgeon to the Birmingham Hospital for Women, &c.

I FIND that in the early days of anæsthesia, contrivances were suggested for warming the ether, and for drawing or forcing a current of air through it, with the intent of securing a saturation of the air with the vapour of ether. But I cannot discover that any suggestion has been made that the ether should be boiled, and that the vapour should be given unmixed with air.

In the *Practitioner*, I have already narrated a case which was the first chloroform accident I had ever witnessed, but which was quite enough to convince me that however much chloroform may be commended for its convenience, we are bound to discontinue its use if another anæsthetic is known to be more free from risk. For the last two years, I have had ether given in all my ovariotomies, and, indeed, in all operations where it was at hand. In the case referred to, chloroform had to be used because we had no ether.

About the time that my friend Mr. Jessop noticed in the *Lancet* that patients might suffer from the after effects of ether in the shape of pulmonary inflammation, I had a case of severe bronchitis in an aged person after the administration of ether. A few weeks after, I had another experience of a similar kind, and then became convinced that ether was not free from risk, and that the source of danger was the loss of heat on the pulmonary surface by the admission of air and ether vapour at a very low temperature.

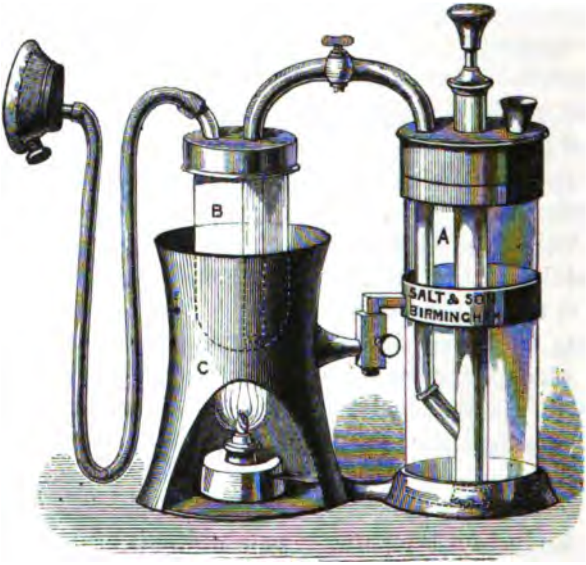
That the mixture admitted to the chest during the administration of ether is at a temperature far below 0° C. may easily be demonstrated by a thermometer, but it may be seen by the abundant coating of ice which is nearly always formed on the towel. This condition is increased in proportion as the ether is the better fitted for the purpose of anæsthesia, that is the lower its boiling point is; so that really our best anhydrous ether is the most dangerous. Another objection arising from this extreme reduction of the temperature, is that the towel gets clogged up with frozen water, thus interfering greatly with the passage of the vapour, and by allowing the ether to take up at once a quantity of water, at the same time it greatly hinders its ready evaporation.

I do not think that any other conclusion can now be drawn from the evidence before us, than that chloroform must be discarded for everything but operations on young children and pregnant women, in both of whom it acts better than any other anæsthetic, and with whom it seems to be wholly devoid of risk. With the single exception of the occurrence of pulmonary inflammation, ether seems free from danger, and therefore its use ought to become general.

Having twice experienced the danger from bronchitis, I tried various plans for removing what I believed to be its cause, and that which I have found to be the best, is to give the vapour of boiling anhydrous ether, pure and free from any admixture of air.

The wood-cut on the next page will explain the apparatus which I have devised for the purpose, and which is made by Messrs. Salt, of Birmingham. Anhydrous ether (·720) is placed in the reservoir A, which will hold about ten ounces, and which is furnished with a spring pump, which drives over about a dram of ether at each stroke into the glass boiler B. This boiler is suspended in a hot-water tank C, beneath which is a spirit lamp. From the boiler an exit tube, four or five feet long, passes to a Junker's mouth-piece.

When the apparatus is to be used, the tank is filled with water, the spirit-lamp is lighted, and about three drams of ether is pumped into the boiler. Care must be taken that there is no leakage from the boiler, otherwise there will be an explosion. It will soon be found that the boiling ether gives over a large volume of vapour at a constant temperature of a few degrees below the boiling-point of the sample, which of course will vary very much, but will generally be found to be pretty nearly the temperature of expired air, 31°—33° Cels. When given at this temperature, and free from air, the vapour is quite pleasant, and its taste, or rather its comparative freedom from taste, reminded me when I tried it on myself, greatly of the flavour of nitrous oxide. It is, of course, certain that its administration will involve no risk of bronchitis. A few days ago, I had to remove the breast of an old lady who had suffered for years from bronchitis and dilated heart. My friend Mr. Priestley Smith gave the ether for me by means of my new inhaler, and we were all charmed by the ease with which the patient passed over into profound coma, and the comparatively small quantity of ether which was needed—about five ounces; and I am pleased to say she has perfectly escaped the complication which I most dreaded,—suffocative catarrh. Although the weather had been horribly foggy and cold ever since the operation, she has not had any cough at all, and has been able to lie flat down in



bed. From my previous experience, I could not have been induced to allow this patient to incur the risks of the administration of ether in the ordinary way,—I should have preferred to use chloroform.

The advance which we have made in our knowledge of anæsthetics since 1849, is in no way commensurate with the enormous amount of labour which has been devoted to the subject. We have, it seems to me, made no advance on ether for surgical work, and Simpson's establishment of chloroform for the alleviation of the pangs of labour stands just where he left it.

I think the method I have detailed will be found to be a substantial advance in the method of ether administration; and if care be taken to keep the boiler vapour-tight, and the mouthpiece three or four feet away from the spirit flame, I think the apparatus will be found quite safe. Dr. Lauder Brunton made to me the ingenious suggestion of having a steam-jacket round the boiler, but that I have not found to be practicable without greatly complicating the apparatus. The use of a hot iron bolt to boil the water, instead of the lamp, would probably be safer, but it would not be so handy, and would involve great wear and tear.

I am indebted to Messrs. Salt for the pains they have taken to carry out my plans.

ON THE INFLUENCE OF SEA-VOYAGES ON THE HUMAN BODY, AND THEIR VALUE IN THE TREATMENT OF CONSUMPTION.

BY C. FABER, M.D.,

Late Res. Med. Officer to the German Hospital, London.

SEA-VOYAGES as a therapeutic remedy, especially for consumption, are resorted to more and more every year by the medical profession of this country. On my return a short time ago from a voyage round the world I even heard of a project to fit out a ship exclusively for invalids, and take them to Australia and back again.

Out of the numbers of medical men who are sending patients to sea for their health, only a few have had the opportunity of personally acquainting themselves with the several conditions which constitute sea-life, and the mode of action of this undoubtedly powerful agent. So they can often have nothing but a vague notion of the *pros* and *cons* regarding the use of this remedy in a given case. But nothing certainly is more important for the successful treatment of a disease than a clear insight into the indications and contra-indications of the remedies used. With regard to sea-voyages, a knowledge of the contra-indications is especially needful, for in their case I have seen that want of accurate judgment which is the necessary consequence of want of accurate knowledge of the remedy and the mode of its action, followed by unusually sad consequences. Imagine a patient torn away, by medical orders, from his family and friends, and all the home comforts doubly dear to an invalid, sent away on a sea-voyage to the antipodes, which, instead of restoring him to a state of comparative health and renewed strength, only hastens his death. How much milder would the pangs of death have been, how much easier the parting from life, had he been allowed to die at home, instead of many thousand miles away on the solitary ocean, in the small, cheerless cabin of a ship!

So the question put to the medical adviser is a double one, involving not only a consideration of the patient's recovery, but also one of sheer humanity. The answer to the question will be in a great measure right or wrong, according to the medical man's correct knowledge of the remedy or his want of it. A careful and accurate judgment is all the more important here, because by the very use of this remedy the physician loses all control over his patient for many months together, and because the remedy cannot be discontinued like any other, in case it turns out injurious instead of beneficial. For to believe that sea-life *must* prove beneficial to everybody is a dangerous fallacy.

My own experience, on which these papers are based, is taken from two long sea-voyages I have made as a medical officer, the first one of over three months and a half, to South America in a mail steamer; the second, of eight months and a half, round the world *viâ* Australia in a sailing vessel. The route to Australia and back is of all others the one most patronised by the medical profession of this country, and therefore most frequented by invalids. In fact, my last ship was full of them, and resembled a regular hospital ship for consumptive patients. As I myself do not expect to get any more medico-nautical experience, and as on the other hand those medical men who are regularly "trading" between this country and Australia appear to be very chary of communicating their more prolonged experience in this way for the common use of the profession and of the class of patients concerned, I may be justified in bringing forward a subject which certainly is not wanting in scientific, as well as practical interest.

I have been endeavouring to extend my observations to every point bearing upon the question of the influence of sea-voyages on the human body, and especially on consumption, as the following list of contents of the different papers in which I intend to deal with it may show.

I. *On sea-climate.*—Sea-climate in general, and the climate of the different oceans and latitudes in particular, as shown by my meteorological journal, where the general state of the weather, the maximum and minimum temperatures, the readings of the barometer, rainfall, direction and strength of the wind, etc., are noted for every day.

II. *On sea-life in its sanitary aspects* for sailors and for passengers, especially for invalids.

III. *On the influence of different climates and of sea-life on the temperature of the body, the pulse, and respiration.*—The results I have arrived at in this interesting question, based upon tables extending over a range of nearly 110° of latitude, and of 360° of longitude, will be found in a good many points materially to differ from and to add to former observations.

IV. *On the physiology and pathology of sea-life*, i.e., in what way the functions of the human body are influenced within and beyond the limits of health, what the prevailing diseases and ailments at sea are, and how far the usual course and signs of diseases are influenced or modified by sea-life, as shown by an analysis of my medical journals.

Sea-sickness is treated in a separate paper.

V. *On the climatic treatment of consumption* in general, and in particular on the question, whether to send a patient on a long sea-voyage or not.

VI. *On the choice of the sea-routes and of the vessel* (steamer or sailing vessel) best suited for a therapeutical purpose, with practical hints regarding the outfit of an invalid, the accommodation and the life on board.

VII. *On the climate, the sanitary conditions and institutions of Australia and Tasmania;* with practical advice to invalids, how long and where to stay or not, and how to live on land.

I.—ON SEA-CLIMATE.

The details, and especially the figures I am going to give in this paper, may appear very dry and trying to read, yet I daresay that he who is sending patients to sea, or who is himself going on a long sea-voyage for his health, will find them of great practical importance. Before, however, going into details, it may not be superfluous shortly to speak of sea-climate in general, and to point out the difference between land- and sea-climate as far as it bears upon our subject. Of the different components which constitute that *ensemble* we call climate, some have very much the same value at sea as on land, *e.g.*, the temperature of the air and the pressure of the atmosphere; whilst others have a peculiar significance at sea. The wind, its direction, and strength is not only an all-important element to the sailor, but also to the invalid seeking health on the ocean. Here it is not impeded or diverted by any visible obstacle which might give shelter; it sweeps free over ocean and ship, chilling or bracing, cooling or scorching, as the case may be. If it is saturated with moisture, there are no mountain-ranges to condense that moisture into rain, so as to dry the air beyond them. On the contrary, if the wind is of some strength it will raise the surface of the sea into billows, the white crests of which send a briny spray over the deck, and at any rate in a purely mechanical way saturate the air on deck with salt-water, even when this is not visible. Too much stress has, in my opinion, often been laid on this circumstance with regard to the therapeutical action of the sea-air. Apart from the moisture raised by mechanical force, that raised by way of evaporation may *à priori* be expected to be, on the whole, larger in amount at sea than on land. This fact appeals to the senses most forcibly under a tropical sun, if there is no wind to relieve the distressing effects of the heat and dampness combined.

Of great moment to animal and vegetable life is the transparency of the air, and in connection with it the *intensity of light.* On the bright, sunny shores of the Mediterranean, many an invalid has regained his health and vigour. In the Alps, high up towards the limit of vegetation, one is surprised to find an abundance of flowers of the brightest hues; although the mean temperature in those heights must necessarily be low. High temperature and intensity of light are not by any means necessarily associated; they may be quite independent of one another. In the tropics, however, they are combined. Here in most places not only high temperature, but also great transparency of the air and intensity of light obtains. The sky seems to be higher, the stars seem remoter and of a mild planetary lustre. Also at sea, especially within the trade winds, one has an opportunity of admiring this transparency of the air, which gives rise to the most brilliant colours, the most delicate tints unknown to our temperate zone. The reverse picture, of the muggy mist of a tropical calm, will be described later on. Taken on the whole, the transparency of the air and intensity of light may be expected to be less at sea than on land in proportion as the atmospheric moisture is greater on the former. On the other hand, fogs such as cover the British Isles for some part of the year are rarer and of shorter duration on high ocean, at least in the warmer latitudes. In the far south of the Pacific, however, one may encounter fogs, not to be beaten by those of the English Channel.

Last, but not least, the principal difference between sea- and land-climate rests on the difference of the *substratum*—sea instead of land. Instead of the variety of climates which on land may be found within a comparatively small compass, in consequence of differences in the configuration, *i.e.* elevation, and in the geological formation of a country, in the amount and character of the vegetation above, and of decomposing organic and inorganic matter beneath the surface of the ground, or in the humidity of the soil,—instead of all this, we have on the high sea a vast sameness of the *substratum*. This sameness is only slightly varied—in the first place, by differences of temperature in consequence of currents and difference of latitude; in the second, by the different state of the sea. The latter circumstance certainly has to come in under the head of "sea-climate," and indeed is a circumstance of no little importance for the questions I am going to treat of later on.

In consequence of this sameness the sea-climate, on the whole, is certainly much more equable than the land-climate. Take, for instance, the climate between lat. 50° and 40° N. on the continents of Europe and America, and you will find the greatest variety of climates, whilst at sea you may sail through the whole of the said latitudes often without noticing any remarkable difference.

But in ascribing equability to the sea-climate, the common notion often goes too far, a circumstance which cannot be impressed too strongly upon invalids before going to sea for their health, and still more upon their medical advisers in weighing the *pros* and *cons*. This being a cardinal point in the question of the value of sea-voyages in the treatment of consumption, it will receive due consideration in a subsequent paper (No. V.) There it will be shown that as regards equability of climate many places on shore deserve preference to the sea. Apart from the changes which, with the different latitudes on a voyage like my last, often range from below the freezing-point to the most intense and depressing tropical heat within a comparatively very short time, there are at sea, even *within the same latitude and longitude, often considerable and sudden changes*, which moreover make themselves much more keenly felt on board a ship than on land.

At the same time I have to correct another common supposition, according to which the tropical climate is the most equable; as my meteorological journals show, it is not exempt from the changes just spoken of. I remember, for instance, that one day in an equatorial calm, the air after a heavy rain cooled down so suddenly and to such an extent, that the ladies were sitting on deck wrapped up in plaids, and with rugs round their legs. Nor, according to my tables, is the mean daily fluctuation of temperature any less here than in high latitudes. So it was 6·3° F. between lat. 10° N. and the equator (16 days of observation), 5·5° F. between the equator and lat. 10° S. (12 days of observation), whilst in the extreme south, between lat. 50° to 57° 17′ S. (18 days of observation) it was only 3·6° F. In going out, however, the minimum of *mean daily fluctuation of temperature* was observed in rather low latitudes, being 2° F. only between lat. 20° and lat. 30° S., whilst on the homeward voyage that minimum (2·3° F.) was observed towards the end of it, between lat. 40° and 50° N. The maximum of the whole voyage was 10·5° F., and was observed between lat. 50° and lat. 44° S. In short, looking all through my tables, I cannot find any rule as to the equability of the daily temperature at sea increasing or decreasing from the poles towards the equator, nor any difference in this respect worth speaking of between the oceans of the northern and those of the southern hemisphere.

In order to illustrate the question of the equability of sea-climate, I may be allowed to mention a few particular instances.

On a very fine and calm day, in a sub-tropical sea (lat. 31° S., long. 29° W.), the difference between the minimum and maximum of temperature was 17°. The same difference I observed another time, in lat. 42° S., between the maximum of one day and the minimum in the following night. In about the same latitude, the difference even between the mean temperatures of two following days was as high as 14·5° F. In another instance, between lat. 45-47° S., the temperature fell within four days by 21° F.

Whilst I could not find any definite rule with regard to equability of temperature, the *atmospheric pressure* showed a most decided gradual decrease of fluctuation from the poles towards the equator. On the other hand, whilst the temperature steadily increases from the poles towards the equator, the mean of the atmospheric pressure scarcely presents any such difference worth mentioning. Between lat. 40° N. and lat. 40° S. —that is, within a range of 80° of lat.—that mean for every 10° only ranged between 30·31 inches and 30·03 inches, the former having been observed between lat. 40° N. and 30°, the latter between lat. 10° N. and the equator. Beyond lat. 40° that mean did not reach 30 inches, being 29·9 inches for lat. 40-50° N. and for lat. 40°-50° S. 29·495 inches, between lat. 50° and 57° S. 17 inches. The total mean for the northern hemisphere (lat. 0°-50°) was a little higher than that for the corresponding range of the southern hemisphere (30·134 inches, against 30·066 inches).

Of the different oceans and latitudes—the climatic features of which I am now going to give in a general outline—there is not much to be said of the temperate region of the North Atlantic. Few do not know the English Channel; for the Bay of Biscay and the outlying high ocean one has only to add in his imagination a good deal more of boisterous weather, of strong winds, and especially of rough sea; and he has an idea what a hard, disheartening beginning a poor invalid is only too likely to have to get over. Wind and weather in this ocean are, however, so variable, that it would be of little use to detail the meteorological experiences of the four voyages I made over it. At one time, in winter, I found it bitterly cold and stormy; another time, in the beginning of summer, temperate and bracing; a third time, in autumn, already from off Cape Finisterre, the climate was most genial and most tropical.

The boundary between the *temperate* and *sub-tropical zone* of an ocean is naturally still less marked than it is on land. It may, however, for the Atlantic Ocean, both south and north of the equator, be put down at about the 35th degree of latitude. Only one must not expect always to find warm weather in those regions; for not only here, but also in the very tropics, it may feel quite cool—more so to one coming from a warm climate. Beside the *cool weather*, two other varieties of weather or climate may be distinguished—the *trade weather*, within the region of the north-east and south-east trade wind—the most perfect climate on earth—clearer, brighter, richer in splendid colours and glorious tints than anything we have in our warm latitudes, and yet very pleasant and enlivening; and in the third place, the *calm weather*. The heat here may be quite tropical as shown by the thermometer; but, the atmosphere being dryer, and consequently the perspiration freer, it is much more bearable.

Our invalid will have to encounter these different sorts of climate to a different extent, according to the class of vessel (steamer or sailing vessel), and the route he is taking (outward or homeward bound). In a steamer he gets over that worst sort of weather—the calms—not only much more quickly, but also with less discomfort to body and mind, there being always motion, and consequently some cooling draught of air.

On the boundary between the temperate and sub-tropical zones of the Atlantic on either side of the equator there is a belt of calms and light variable winds, which in the North Atlantic is called the horse-latitudes.

The homeward-bound track of sailing-vessels, making a large curve to the westward from about the equator on account of the north-east trade, and in order to get into the westerly winds of the North Atlantic, goes right through those horse-latitudes. So, coming home last time, we lay here becalmed for a whole fortnight without scarcely moving.

A phenomenon, which I noticed particularly in these sub-tropical zones, but which is occasionally also to be met with in higher latitudes, especially after a calm and hot day, requires particular attention on the part of our invalid—I mean the *heavy dewfall* which, together with a sharp, piercing breeze, sets in about one hour or half an hour before sunset, and continues for some time after it. I have seen a good many fresh colds, pleuritic affections, neuralgia, and minor ailments, arise from invalids exposing themselves during this time on deck.

In the *tropical* part of the Atlantic on either side of the equator we may distinguish the same three varieties of weather as we have done in the sub-tropical zones. The cool weather, with the sky overcast, and usually with strong winds, may be continual throughout, or it may only interrupt, often all of a sudden, the damp, oppressive heat of the doldrums (equatorial calms), following a heavy thunderstorm and torrents of rain. More often, however, the *frequent heavy rains,* which are characteristic of the doldrums, only serve to render that heat still more oppressive and distressing. In such a calm one even prefers the sun glaring down, with all its tropical intensity, to the dull, muggy, leaden picture, into which sky and sea blend on rainy days. This is another and, I dare say, the hardest climatic trial our invalid has to undergo. The most pleasant time in this climate is the night, and here even invalids may stay up on deck until late in the night with impunity, and they often even sleep on deck under a tent, although I should not recommend the latter.

The climate of the trade winds has been already described; it is, of course, warmer within the tropics, but still pleasant. The trades vary in extent and in strength with the season—*i.e.*, with the march of the sun towards the one or the other tropic.

In the tropics, the highest temperature I observed at sea was going out, 85° F.; coming home, 87° F.; the former north, the latter south of the line. The mean daily temperature within 10° to the south and north of the equator (28 days of observation) was 80° F. The rule that the southern hemisphere has a lower temperature than the northern did not yet show itself in the tropics; the mean temperature within the 10th and 20th degrees to the north and south of the line being, on the contrary, as 77·40° against 78·96° F. But in the sub-tropical, and still more in the temperate regions of the Atlantic, that rate was confirmed. For instance, a mean daily temperature of 20-30° F. was observed at lat. 74·7° N. and lat. 72·2° S.; of 30-40° F., at lat. 68·64° N. and lat. 63·6° S.

These figures are the more to be relied upon as the months of observation in the two hemispheres were nearly or entirely corresponding ones. So, for instance, the months of October and May, when I passed lat. 20-30° N., exactly corresponded to November and April, when I passed the corresponding southern latitudes.

The temperate region of the *South Atlantic* has in its general character a good deal in common with that of the North Atlantic. The difference in temperature between the subtropical and the temperate region seems to be more marked in the south. The prevailing westerly winds are usually stronger, and more to be depended upon. To run before a fresh westerly gale, with a rough sea and a clear, cold sky, is the picture the sailor wishes to see realised in this part of the ocean. In fact, the climate here is, on the whole, fine and bracing, if one is only hardy enough to stand a continually rough sea and an occasional sharp cold. Here it was that in coming home, on the 7th of April, to the eastward of the Falkland Islands, I observed the minimum temperature of the whole voyage—39° F. It is remarkable that at the same time a northerly, not, as might have been expected, a southerly, gale was blowing. In going out, whilst still on the South Atlantic, but already close to the meridian of the Cape of Good Hope, in lat. 42° S., the thermometer fell to 42° F., which was only 1° F. higher than the minimum for the voyage out, which I observed during a fresh west-south-westerly gale about in the middle between the meridians of the Crozets and the Kerguelen Islands in the South Indian Ocean.

This ocean I traversed between the latitudes 41° and 44°. I have got a rather favourable impression of it, as I have of the South Atlantic; the climate is fine and bracing, only the temperature is still lower. The mean temperature of the twenty days we took from the meridian of the Cape of Good Hope to that of Cape Loenwin in Australia was 53·45° F. By the way, I may remark here that the figures I give in this paper refer to the very months in which invalids usually make their voyage to Australia and back, leaving England in the beginning of October, and coming back in June.

Of the South Indian Ocean my impression, no doubt, would have been less bright if we had gone further to the south, *as ships according to usage and the direction of the wind frequently do.* Then I might have had to register intenser and more continual cold, dull, occasionally rainy and foggy weather, and even the appearance of ice in fields and bergs. An invalid sent out on a sea-voyage to Australia in a sailing-vessel must, therefore, be prepared to encounter such a climate.

The South Pacific Ocean is like a grey, dreary blank in my memory—stormy, cold, and wet—"Scotch mist" preventing the sun for many days together from appearing and giving a more cheerful appearance to the vast desert of water. At the outset, however, near the Australian continents and Tasmania, the temperature kept remarkably high; and even between lat. 49° and 50° (16 days of observation), the mean temperature was still as high as 53° F., the extremes being 42° and 65° F. The weather during this period, with its frequent and sudden changes from warm to cold, from dry to wet, quite reminded one of our spring weather, and also had the same effect on the people on board.

The mean temperature out of twelve days, between lat. 50° and our south point (57° 17′) was 45·2° F.; the highest temperature, 49° F.; the lowest, 42° F. The mean atmospheric pressure was 29·53 inches, against 30·13 inches in the period mentioned just before.

(*To be continued.*)

Reviews.

Archives of Dermatology: a Quarterly Journal of Skin and Venereal Diseases. Edited by L. DUNCAN BULKLEY, A.M., M.D. First volume, 1875. New York: Putnam and Sons.

THE numbers of this new dermatological journal that have appeared, and which constitute the first volume, sustain well the promise of being a complete repertory of all current work in its own department with which the *Archives* made its appearance a year ago. The staff of "collaborators" includes a number of well-known names amongst American dermatologists, and we find contributions from England and Vienna in these first numbers. The record of contemporary work is as complete as it can well be, nothing that is published in Europe or America on skin diseases or syphilis being overlooked.

Clinical Lectures and Essays. By SIR JAMES PAGET, BART., F.R.S., D.C.L. Oxon., LL.D. Cantab. Edited by HOWARD MARSH, F.R.C.S.

THIS book is one of the most valuable contributions to the literature of clinical surgery which has appeared for many years. It is always a matter for congratulation when men of large experience find time to publish the records of their observations: and this is especially the case with regard to Sir James Paget, whose well-known powers of accurate observation and reasoning make all his communications worthy of our most careful attention. The volume under notice contains, besides some clinical lectures which have already appeared in the journals, several most suggestive essays, and some "Notes for the Study of Constitutional Diseases," furnishing valuable material for reflection and inquiry. It is written in classic English, and is characterised by a clearness, comprehensiveness and modesty, which render it a model of style and method. The author apologises for his apparent disregard of the works of others, but we think that the chief value of the work consists in the fact that it gives us his personal experience and opinions. Mr. Howard Marsh has, however, besides editing the book, added some very useful and interesting notes.

The essay on the various risks of operations gives many useful cautions, and shows that the best recoveries are made by boys between the ages of twelve and sixteen years, and by invalids who have lived carefully and temperately, and whose mode of life is not much altered by the operation. The worst subjects are those who eat and drink too much, and those who are fat, flabby, and torpid, and whose digestive powers are not equal to any extra call. Closely connected with this is the subject of the next chapter, "The Calamities of Surgery." The author points out how many of these calamities may be avoided by careful attention to details, and how others are not by any means to be averted. The essay "On Stammering with other Organs than those of Speech," explains that "stammering, in whatever organs, appears due to a want of concord between certain muscles that must contract for the expulsion of something, and others that must at the same time relax to permit the thing to be expelled"; and in this way difficulties may arise in micturition, deglutition, or defæcation. Mr. Marsh appends an instructive case in which there was stammering with the organs of speech as well as with those concerned in defæcation. "Cases that bone-setters cure" are next treated of, and are shown to be chiefly those of joints stiffened by old adhesions. These adhesions may often doubtless be ruptured with much benefit, and the author gives us the result of his experience to aid us in the selection of fit cases for this treatment. Then follow some lectures on Strangulated Hernia, full of practical information and advice. We are glad to see that Sir James Paget insists on the value of vomiting as a symptom commanding the operation; we believe this to be the most certain guide in doubtful cases, and a symptom which should outweigh many negative signs. Mr. Marsh adds some very interesting notes on the peritonitis occurring in connection with strangulated hernia, on the aspiration of hernia, and on the question of opening the sac. With regard to the last point we feel bound to differ from the editor, and we think that a very strong argument in favour of opening the sac is afforded by the fact that one of the greatest sources of danger in operations upon the abdomen arises from the presence of morbid products in the peritoneal cavity, and that if the sac is not opened, such products must be returned into the abdomen instead of escaping externally. "Chronic Pyæmia" is the subject of the next paper. Several examples of the disease are given, from which it is seen that such cases, while resembling in many of the essentials the typical form of the disease, yet "differ from the acute type in that their course extends, continuously or with relapses, over many weeks and months, or even over years, and is often free, at least in its later stages, from all severe general disturbance of the health, and from nearly all risk of life." There are six most valuable lectures on "Neuromimesis" or "nervous mimicry," in which the sagacity of the author is very notable. It is well pointed out that the subjects of this affection are persons of unusual mental or emotional sensitiveness, and often good, wise and accomplished; not by any means all silly or fraudulent. It is a malady of the nervous system, and the family history often reveals the existence of allied affections or even insanity among the relatives. The author gives many useful guides for distinguishing the real from the mimic disease, and for the treatment of the latter. The lecture on the Treatment of Carbuncle teaches that the surgeon should confine himself to the careful and cleanly dressing of the wound, and to securing for the patient plenty of fresh air. The old plan of cutting, stimulating, and confining to bed, persons with carbuncles is shown to be erroneous and harmful. Most admirable is the essay on what Sir James Paget has called "Sexual Hypochondriasis." The cases included under this name "are those of male patients who regard trivial maladies, or even some of the natural events, in their sexual organs, with the unreasonable dread or gloom and watchfulness which are characteristic of hypochondriasis." The author treats this difficult subject with characteristic insight and skill, and we are glad to have so weighty an authority to quote against the harmful and wrong teaching which he so justly denounces. The next paper describes that form of phlebitis which "occurs with little or no evident provocation in persons of marked gouty constitution, or with gouty inheritance," and which is characterised by its symmetry, apparent metastasis, and frequent recurrences. An excellent description is given of "residual abscesses" or "abscesses formed in or about the residues of former inflammations;" and there is also a paper on cases of "quiet necrosis," in which the death and exfoliation of bone take place without any of the usual inflammatory phenomena. An essay on "Dissection Poisons" is founded upon the author's study of his own symptoms when suffering from the results of a dissection wound. It is pointed out that a constant contact with this kind of poison gives an immunity from its effects; but that the susceptibility is regained after a period of freedom from such contact. The chapter on Scarlet Fever after Operations draws attention to the important fact that there is a peculiar susceptibility to the poison of scarlet fever in those who have recently undergone surgical operations; and Mr. Marsh adds, in a note, some valuable facts on this subject. The book ends with the "Notes for the Study of some Constitutional Diseases" to which we have already alluded; and which, although only professing to be fragmentary, give evidence, like the sketches of a great painter, of the wealth of material from which they have been drawn, and exhibit very notably the master's touch.

Lectures on Syphilis, &c. By HENRY LEE, Professor of Surgery at the Royal College of Surgeons of England, &c., &c. London: J. and A. Churchill, 1875.

IN this work the author has endeavoured to illustrate Hunter's doctrines in regard to syphilis, and has taken an opportunity of discussing the pathology and treatment of syphilis, urethral and prostatic disorders, and other local affections of the genito-urinary organs. To the fulfilment of his task he has applied a wide and philosophical comprehension of the general and special pathology of the subject, one which his own extended personal experience gives him a strong right to handle.

In the three first lectures he deals with the evidence of blood contamination and inoculability, during the primary and secondary stages, and deduces conclusions in favour of the view that the results of secondary manifestations are inoculable so as to propagate the disease.

As is well known, the author is a supporter of the use of mercury, and has great admiration for the calomel vapour bath, and minute directions are given for the employment of that valuable mode of treatment. He commences mercurial treatment for the primary affection, *i.e.*, the chancre, and advises its maintenance for some time after the disappearance of induration and of the secondary symptoms—usually a minimum period of three months. We confess we prefer to have some general evidence apart from the local lesion to satisfy us of the necessity for a mercurial course. If we sanction the so-called preventive treatment we run a certain risk of inflicting unnecessary medication, and, in our opinion, are following an unscientific rule-of-thumb method of practice. The diagnosis of a sore which will infect the system, from the local sore which will not, is a frequent difficulty with the best observers.

Mr. Lee occupies the position of a "dualist." He avers with reference to the soft chancre or "local suppurating venereal sore" (p. 192) that he has *never* known it to be followed by secondary symptoms. We believe that this occasionally does happen, and we are not singular in that view. Nor do we repose any confidence in the ingenious hypotheses regarding mixed sores which are in vogue among the so-called dualists, who thereby seek to explain away the occurrence of infection after chancres which are not distinguishable in themselves from local sores. Mr. Lee believes in a specific infecting sore, and in a specific non-infecting sore. Both are venereal—each is specific. One infects the system—the other never does. The one only reproduces its kind on another subject who has never previously contracted it, the other reproduces itself freely on the original patient, or on anyone with whom he comes in contact. We are opposed to this view, and we affirm that the one is merely a modification of the other. The following is the argument on which we place reliance.

Mr. Lee and others have over and over again inoculated the secretion of hard chancre on the person of the same patient and also on other syphilitic subjects. What has he found? Has he not found that such inoculations resulted either in failure to "take," or in the production of the "*local suppurating* sore?"—at least if *not* the local sore, the sore so resulting is quite indistinguishable from it. Now it is a fair question, if the above is a correct apprehension of observed facts, whether it is probable that the two sores are essentially different. Is it not fair to infer that the difference lies in the conditions as to receptivity, viz., whether the subject has or has not had constitutional syphilis? We submit that at least the evidence points to some connection, and if so, what? We might here speculatively suggest that it is an instance of allotropism in pathological chemistry. Whether the virus is twofold or single, there can be no doubt that similar conditions are involved in its primary generation—to wit, indiscriminate congress, aided by filthy personal habits, and maintained by transmissible vitiation of blood and secretions. But we refer the reader to Mr. Hutchinson's account of this matter in his recent address on syphilis. His account of the relationship of the two sores we believe to be the most reasonable ever advanced.

In treating some of the secondary manifestations, Mr. Lee professes a confidence in sarsaparilla which would have delighted "old Dr. Jacob Townsend," who, by the way, ought not to be dead yet if his account of the drug was correct.

The seventh lecture contains much interesting matter in connection with lymphatic absorption, a subject which is resumed again in the final lecture. The enlarged condition of the glands is we think regarded by the author as the only reliable evidence of the presence or absence of (? recovery from) the taint. But for the difficulty arising from the too frequent co-existence of scrofula and syphilis, we might agree with him as to the value of this test in aiding us when asked to advise the syphilitic about fitness for marriage. This important and difficult question deserves more elaborate treatment at the hands of systematic writers than it has received. It is one of the greatest moment to the community and hinges of course on the primary question whether syphilis is *curable in the fullest sense* or not. We believe that a *successful* Contagious Diseases Act working through prevention is the only therapeuticanswer to this question.

Mr. Lee deals only cursorily with visceral syphilis. As to tertiary conditions affecting the viscera, and, nowadays at all events, the most important part of the subject so far as the mortality from acquired syphilis is concerned, we are probably only on the threshold of startling pathological discoveries. The writings of Buzzard, Moxon, Wilks, Broadbent, and others point to remote syphilis as a more frequent cause of death than the practitioner of say 15 or 20 years' standing could have imagined. And we think the confirmation of their views should be full of meaning for those who have charge of the interests of insurance societies, although the accurate data from which practical deductions could be drawn are not yet forthcoming. We do not think anyone who has ever had syphilis should be accepted as a first class "life."

The limits of this notice forbid further detailed allusion to Mr. Lee's book. Notwithstanding the occasional want of method in the arrangement of his matter, inseparable from such a treatise when it takes the form of collected lectures—a perusal of it cannot fail to repay the reader, who will find in it reports of many interesting cases and experiments, together with much suggestive philosophical discussion—not to say quotations from the poets—an embellishment we believe rarely accorded this particular subject.

Clinic of the Month.

The Influence of Climate in the Treatment of Pulmonary Consumption.—This subject has been selected by Dr. Theodore Williams for the Lettsomian Lectures. In his second lecture, he gives interesting details of the isothermal lines of the southern district of England, showing that they vary considerably in direction with the different seasons, and further that the British Channel varies greatly in its winter climates, so that in proceeding from the Land's End towards the Straits of Dover the temperature gradually falls. In the month of January in the Scilly Islands, it is 46° F.; at Helston, in Cornwall, it is 45°; at Torquay, it is 42°·4; at Ventnor, 41°·6; at Worthing, 39°·6; and at Eastbourne, 39°·3 F. He finds the rainfall varies from 23 to 44 inches, according to the proximity to mountains, and the number of rainy days from 132 to 178. The prevailing winds are westerly. He proceeds to give the details of 243 consumptive cases, of which 135 were males, and the general result of his observation is, that, all circumstances taken into consideration, cases of pulmonary consumption do better in the eastern and colder than in the western and warmer watering places. (*British Medical Journal,* January 22, 1876.)

Sudden Death after Injection of Iron in Post-partum Hæmorrhage.—Dr. Cederskiöld reports a case where a woman, after the birth of her second child, was seized with flooding. The uterus did not contract fully, and its fundus could be felt over the pubes. Ergot was tried without avail, and the bleeding recurred from time to time. Eighteen days later a solution of perchloride of iron, one part in seven, was injected into the uterus. When the injection was half completed the woman complained of pain, and, after a few breaths, died. On making an autopsy next day, it was found that the small intestines were congested. The peritoneum in the neighbourhood of the fossa of Douglas was found studded with black spots. The uterus was rather firmly contracted, and its interior stained dark brown. It was uneven, and covered with reddish granulation tissue. Three superficial oval ulcerating surfaces were found from one to two inches long at the fundus and sides; here the substance of the uterus was exposed, and had a ragged surface, where open-mouthed vessels were seen, some of them large enough to admit a fine sound, and which passed into the veins of the uterus. These were followed up into the hypogastric veins. Bubbles of air were seen in these and in the right side of the heart. (*Swedish Med. Society*, and *Med. Press and Circular*, January 11, 1876.)

On the duration of Quarantine required after Puerperal Fever.—Dr. Braxton Hicks states it has frequently been urged that an opinion should be authoritatively pronounced for a guide to the profession generally, as to the length of time an accoucheur should abstain from midwifery practice after attending a case of puerperal fever. The public, he thinks, have acquired an idea that it requires three months' purification before a medical man is fit again to attend confinements. It is obvious that no statement of this kind can have originated from the profession, as, were the same rule to exist after all cases of zymosis, the impossibility of practice is clear. He has always held himself that *three weeks' absence*, and purification, are the best approximation to the requirements of the case, giving ample time—probably more than what is actually necessary—to discharge the poison from the system and clothes. This was also Dr. Lever's opinion, and its soundness has been borne out by long experience over very large fields of observation. (*British Medical Journal*, January 22, 1876.)

Notes on Syphilis.—Mr. Jon. Hutchinson opened a discussion on syphilis by the Pathological Society with an address, in which he took occasion to remark that he considered it was generally accepted that the phenomena of this disease, however various, are due to one virus, which having been introduced into the body by contagion, develops within it, with tolerable uniformity as regards stage and time, certain processes of inflammatory growth. The dualistic doctrine of syphilis he thought was dead, soft cancer being due to contagion with inflammatory products produced by syphilis, but not as a rule containing its germs. Syphilis he regards as a specific fever of prolonged, but definite stages, produced by contagion only, which has a period of incubation, of outbreak, and of efflorescence or exanthem, and is in exceptional cases followed by sequelæ, to which the name of tertiary symptoms is given. These tertiary symptoms are never general, and only by accident symmetrical. They are in all probability not capable of spreading syphilis, though a phagedænic sore may spread phagedæna. The products of syphilitic inflammations are very peculiar. From beginning to end—from the chancre to the latest tertiary gumma—the tendency to cell-growth is most remarkable. In addition there is little tendency to suppuration, a tendency to cause death of the tissues affected, and thus to produce phagedænic ulceration or even sloughing, and there is a proneness to undergo rapid and complete absorption, especially when attacked by certain metals and their salts. Relapses are however frequent. The primary chancre, the secondary rash, and tertiary gumma are all alike liable to undergo molecular death and to be attended by phagedænic ulceration, and phagedæna once started exists on its own behalf and spreads by the contagion of its own pus. To arrest its spread it is necessary to destroy its secretion. With few exceptions we may count syphilis either directly or indirectly as the parent of all phagedænæ. Mr. Hutchinson thinks that the visceral pathology of the secondary stage of syphilis has been far too much neglected, and that hereafter many important facts will be obtained. In the secondary stage all the morbid processes are transitory and tend to spontaneous cessation; whilst the tertiary ones are more persistent and tend to distribution of the parts concerned. He gives various facts derived from the eye, ear, heart, testes, spleen, and bones, which *discredit* the opinion that certain tissues are attacked in the secondary and certain others in the tertiary one. Finally, he contrasted the course of inherited syphilis with that of the acquired disease, alluded to the laws of hereditary transmission, and discussed the probability of syphilis mixing itself with other causes of disease, and producing scrofula and lupus. In the discussion following Mr. Hutchinson's paper, Sir James Paget remarked that in answer to one of Mr. Hutchinson's questions, whether there were any facts which favour the belief that syphilis continues to be a blood disease after the cessation of all tendency to produce symmetrical symptoms, he was strongly of opinion that it continues to be a blood disease up to its later stages, illustrating in the best way the manner in which a blood disease gradually ceases, whether by diminishing in intensity, or the quantity present in the blood becoming irregular, and at last ceasing to be a blood disease, and losing all specific character. He agreed with the view that the gummata of the tertiary stage result from renewed growth in forms left over from the exanthem stage, or that, expressing it otherwise, the exanthem stage of syphilis leaves behind it certain residues in which the disease may be renewed by any casual external conditions. Sir James Paget does not consider that syphilis predisposes to scrofula or tuberculosis to any greater extent than any other fever, nor does he think that lupus is in any sense a syphilitic disease. He in this, as well as in the belief that there is but one syphilitic virus, fully agrees with Mr. Hutchinson, and attributes the various phenomena that are observed in different individuals to the various soils in which the poison has to develop and grow. (*Lancet*, and *British Med. Journal*, Feb. 5th, 1876.)

On Treatment of Convulsions in Infants.—Mons. Blanchez, in a lecture on diseases of children, lays down the following rule for the treatment of convulsions. If it be a single attack, and gives no signs of a tendency to recur, it is best to confine ourselves to some hygienic precautions such as securing efficient ventilation, &c. If the attacks run into each other, or recur at short intervals, revulsives should be applied to the lower extremities, compresses of cold water or of water with ether being also laid on the temples. Compression may at the same time be made on the carotid arteries, as recommended by Trousseau. The pulsation of these vessels must be sought for at the lateral parts of the neck, and then they must be gradually compressed backwards towards the spinal column. The amelioration should be rapid; and if after two or three minutes it has not manifested itself in an evident manner, the compression should not be longer continued. Inhalations of chloroform may then be resorted to, administering them in a very gentle and gradual manner. In order to avoid all danger, slight as this is, it is necessary that a certain quantity of air should be always mixed with the chloroform vapours. In some cases special indications present themselves, as for the employment of an emetic when it is well made out that the convulsions are due to indigestion. When the attack has been overcome, we must try to modify the general eclampsic condition by having recourse to anti-spasmodic treatment; but the management of agents of this description requires great prudence, several of them being of a dangerous character. Their dose is of great importance. For an infant, from eight to fifteen months old, we should never exceed the dose of thirty centigrammes after having commenced with five centigrammes. The maximum dose of belladonna powder is ten centigrammes after commencing with one, increasing it very gradually, and carefully watching the throat and pupils of the child. We may proceed more boldly with oxide of zinc or James's powder (which M. Blanchez has not found of any special utility), of which ten centigrammes may be given every two hours; but bromide of potassium and chloral are to be preferred to any of these remedies. Of the bromide from ten to twenty centigrammes may be given every two hours until fifty or sixty centigrammes are reached in an infant, and from two to three grammes in a child of seven. The effect should be manifest at the end of twenty-four hours, or the dose should be increased. A mixed treatment of the bromide and chloral gives little better results, the bromide being given during the day and the chloral at night. The maximum of the latter agent is twenty-five centigrammes for an infant, and fifty for older children. (*Medical Times and Gazette*, Feb. 5th, 1876.)

Extracts from British and Foreign Journals.

The Treatment of Venereal Disease with Salicylic Acid.—Dr. G. H. Boyland observes that a decade since no one would have thought of connecting the contagion of venereal disease with the presence of microzymes. The infections of gonorrhœa, phagedæna, balanitis, prostitis, &c., were indeed admitted to be the result of poisoning by means of some physico-chemical action or inflammatory process, spreading not only from one individual to another, but from part to part of the tissue in which it begins; as, for instance, the spread of gonorrhœa along the mucous surface affected by it. Modern investigations have, however, led us to the conviction that this morbid agency is principally due to the existence of lower organisms. If we place upon the object-glass of the microscope drops of the discharge from the urethra of patients passing through the different stages of gonorrhœa or gleet, we find that the micrococci increase or decrease in direct proportion to the severity of the inflammation. In the burning, greenish-yellow virulent secretion of acute gonorrhœa, their numbers literally obscure the view, whilst in the milky, viscid, at times almost colourless discharge attending gleet, their presence, though readily detected, is very much less marked. The same holds good of phagedæna, balanitis, and prostitis. The fact being then estalished that these diseases are of parasitical origin and nature, the *rationale* of their treatment is evidently the destruction of organic matter. That salicylic acid annihilates both animal and vegetable parasites as well as prevents their development, and may therefore be employed with advantage in the treatment of venereal disease, Dr. Boyland has already shown in a paper contributed to the *Lancet*. He gives the details of eight cases in which the salicylic injections were used with the best effect. In the inflammatory stage the strength did not amount to more than 1:200, but in the later stages stronger solutions (1:100) were employed. (*American Journal of Med. Sciences*, Oct. 1875.)

On the Influence of Syphilis in Pregnant Women, under Various Modes of Treatment.—Dr. F. Weber, of St. Petersburg, has given the results of his observations in 129 pregnant women suffering from syphilis admitted into the Obuchow Hospital during the ten years 1863-'73. Of these patients 35 were treated only locally or not at all; 35 were submitted to treatment by inunction; in 23 inunction was combined with the external use of iodine (iodide of potassium with tincture of iodine); 19 were treated by the internal use of a combination of iodide of potassium and corrosive sublimate; and in 17 cases iodide of potassium was the only remedy used. He gives abundant statistical details, and sums up as follows:—In general, the course of pregnancy was interrupted in 25, or 20 per cent. of the cases; this proportion, however, may be reduced, when it is remembered that of the patients four had erysipelas of the head, one recurrent fever, and one exanthematous typhus. 2. Every method of treatment which interferes with the digestive system predisposes to untimely birth. 3. In the cases submitted to simple local treatment, there were 20 per cent. of premature births; in three, however (suffering from typhus and recurrent fevers, and from extensive formation of abscesses), violent fever appears to have been in part the cause of the untimely labour. 4. In pregnant women who were treated by inunction together with local remedies, there was no disturbance of the course of pregnancy. This confirms Professor Sigmund's conjecture, that the inunction treatment has no injurious influence on the course of pregnancy. 5. In women in whom inunction was either accompanied or followed by the internal use of iodine, the percentage of premature births was 37; this, however, may be reduced to 20 by deducting two severe cases of erysipelas of the head. 6. General treatment with a solution of iodide of potassium and perchloride of mercury was attended by 15 per cent. of premature births. 7. In cases treated by iodide of potassium, 42 per cent. of untimely births occurred. 8. The injurious action of general treatment did not in any way correspond to its duration, but much rather to its effects on the digestive organs. Hence general treatment should be interrupted on the first indication of indigestion in a pregnant woman. 9. The period of pregnancy at which general treatment is commenced appears to have no influence on the occurrence of premature labour. 10. The stage of development of the syphilis seems to be not without influence on the occurrence of untimely birth. 11. The puerperal period ran an abnormal course in 4 out of 14 cases treated locally, in 3 out of 8 treated by inunction and iodine, in 3 out of 4 treated by iodine and sublimate (one of these patients died), and in 4 out of 10 treated by iodide of potassium. (*American Journal of Medical Sciences.*)

The Treatment of Rheumatic Fever with Salicylic Acid.—Like all new remedies salicylic acid is being tested in a number of different diseases, and among these of course rheumatic fever occupies a prominent place not only on account of its frequency, but also of its markedly febrile character. The results which have been obtained in Prof. Traube's wards at Berlin, certainly afford not a little encouragement to indulgence in a hope that in salicylic acid a real remedy for acute rheumatism has at last been found. We extract the following details from a paper by Staff-surgeon Stricker, in the *Berliner klinische Wochenschrift* of January 3rd, 1876. For several months all the cases of acute rheumatism in which the local symptoms were strongly marked (fourteen in all), were treated with salicylic acid. The preparation used was, however, not the ordinary impure commercial acid, but one that by repeated crystallisation had been rendered almost perfectly pure. Thus prepared it consists of shining white needles, which have no smell, and dissolve completely in water and alcohol so as to form a clear solution. This pure acid can be given internally in considerable doses without any of those unpleasant results which have followed the use of the commercial acid which probably owes its caustic properties to the presence of other substances,—for instance, carbolic acid. The pure acid only excites some dryness in the mucous membrane of the mouth and pharynx, followed by an increased secretion from their surfaces. This inconvenience can, however, be obviated by giving the acid in half-gramme or gramme doses every hour, in the form of powder and enclosed in a capsule; and in the treatment of rheumatic fever the administration is continued until the joints which were previously affected can be moved without pain. To quote Dr. Stricker's words, "All the patients thus treated were not only relieved of their fever, but also of the local symptoms—*i.e.* the swelling, redness, and especially the painfulness of their joints—within forty-eight hours; most of them even within a much shorter period." The largest quantity of pure salicylic acid which was found necessary to produce this effect was fifteen grammes (230 grains), and the smallest five grammes; but that even larger quantities can be taken internally without injuring the digestive apparatus is proved by the fact that one patient actually took twenty-two grammes in the course of twelve hours through an excess of zeal on his own part; but, nevertheless, his tongue became clean, and his appetite returned in the course of this vigorous drugging. As far as Dr. Stricker's observations go, the more acute the case the better the action of the acid. He finds it best to begin the treatment in the morning, for then its effects are generally so decided by the evening that it is unnecessary to disturb the patient's rest to give him his medicine. The general phenomena which were observed to follow large doses of the acid were copious perspiration, ringing in the ears and slight deafness, and in two cases the patients became more than usually lively. Dr. Stricker does not pretend to express any opinion at present on the effect exerted by the acid on the cardiac complications of rheumatic fever. Most of his cases had either old valvular disease or else were suffering from a recent endocarditis at the time of their admission. The details of five cases are appended to Dr. Stricker's paper, and the temperature-sheets are given in graphic form in four of them, and we can only say that they confirm in a most striking manner what has been above stated as to the value of the salicylic treatment. (*Medical Times and Gazette*, Feb. 5th, 1876, from *Berliner klin. Wochensch.*)

Treatment of Chronic Bright's Disease.—The following is a summary of a plan of treatment recommended at the Bellevue Hospital, New York:—*Diet:* This class of patients should abstain as much as possible from meat. The opinion has been expressed that the excessive animal diet accounts for the great prevalence of the disease in America. Milk should be substituted for meat, and should be associated with lime. Butter may be used; eggs if they agree, and fresh fish in the morning. Fried fats should be carefully excluded, but cream may be taken without stint. Vegetables and fruits are *always* good, but those should be selected which contain the least amount of woody fibre. Rice and potatoes, therefore, may be used, but asparagus, turnips, cabbage, and notably beans, which contain woody fibre in large quantities, should be assiduously avoided. Onions may be eaten with impunity, and are rather beneficial.—*For the Anæmia:* Iron should be administered from first to last, and by preference, the tincture of the chloride. This preparation is assimilated with difficulty, hence should not be given alone, but combined with nux vomica, and to this spirits of nitre may be added to assist the determination towards the kidneys. For example, ten drops of the tincture of the chloride of iron, ten drops of tinct. nucis vomicæ, and one drachm of sweet spirits of nitre may be given three times a day. Cod-liver oil increases the red corpuscles of the blood, because it is digested by the liver, and the product enters into them as an ingredient. The irritability of the stomach may make it troublesome to take, but it should be relied upon as much as in the treatment of phthisis.—*To Combat the Disease Itself:* We have one agent which may be regarded as a specific against increase of connective tissue in the body, wherever the interstitial inflammation may occur, and that is the bichloride of mercury. It should be given in small doses, one-twentieth of a grain is the usual amount, and should be combined with a diuretic to make it act upon the kidneys. For example, one-twentieth of a grain of the bichloride, one grain of digitalis, and one grain of quinine may be given three times a day, with the result of producing a specific action upon the kidneys, and will *raise* the specific gravity of the urine.—*Attention to the Condition of the Skin* will materially assist the embarrassed kidneys, and to do this we may have recourse to two things. If excessive œdema is present, the pressure produced shuts off the circulation to a great extent and prevents removal of the fluid by diaphoresis. It is much better then to make punctures in the distended skin of the legs, and let the water drain away at once. No apprehension need be had with reference to this trifling operation, if the limb, when the punctures have been made, is wrapped with cloths wet in a solution of carbolic acid in water, to which has been added essence or oil of cinnamon. The latter is to correct the smell of the carbolic acid, and is also equally antiseptic. The second thing is, to rub the patient all over once a day with sweet oil. If extra diaphoresis is desirable, it can be best obtained by placing a blanket in an empty bucket, pouring hot water upon it; for in this way much less water is required, and then wringing it out and quickly applying it around the body and covering it with a dry blanket. The skin should be well oiled before the blanket is applied. Such is a brief outline of the general treatment for this class of cases, and may be suggestive in certain particulars.

Action of Quinine.—Dr. Vincenzo Chirone contributes a long paper on the action of quinine to the Italian journal *Lo Sperimentale,* in which he gives the results of numerous experiments he has made with it, both by subcutaneous injection and by internal administration, and concludes by stating that its primary effect is to produce temporary exaltation of the functions of the circulatory apparatus, which is followed by depression. It augments, in the first instance, both the force and the frequency of the heart's action, and the rapidity of the current of blood, and it slightly increases also the arterial tension; and, secondly, it causes diminution of the force of the systolic impulse of the heart with lowered frequency, and an increase of the venous tension in relation to the arterial, with all the consequences of this. The *seat* of the action of quinine he believes to be the muscular fibre of organic life, and the *nature of its action* to be an excitation of muscular extensibility. (*Lo Sperimentale,* Fas. 11, 1875.)

Active Principles of Mais Avaria.—M. Lombroso gives the chemical details of the mode of extraction of certain poisonous principles he has obtained from the Mais avaria. One was a bitter red substance obtained from the tincture, soluble in potash and in ether; it causes death when administered to frogs and pigeons by producing convulsions and clonic spasms. Another mode of preparing the tincture gave a substance that either caused tetanus in the course of from four to seven hours, the animals previously exhibiting hyperæsthesia, or, sometimes a kind of ataxia, which appeared to be due to another substance; when administered to fowls in the dose of from 4 to 12 grammes, for three months, it caused choreic movements. On treating with water the maize that had already been exhausted by alcohol, a third substance was obtained, which in its general aspect, odour, and taste, resembled ergotin. This third substance, when injected into frogs, caused dyschromia, narcotism, and paralysis of the legs, especially of the right side. The frogs could still swim, but they could not preserve their horizontal position; when placed on a table they could not recover themselves; clonic convulsion of the posterior limbs then occurs. The pulsations of the heart are slowed, and death follows in four hours. When given to cats, it causes vomiting, then paralysis with clonic spasms of the feet and muzzle. Death takes place in an hour or an hour and a half. In dogs, when administered by the stomach, paresis of the feet is produced; the animal is unable to leap; when it is irritated it falls forward on the nose; at a later period, spasms occur in the hind feet. If compelled to walk about, it falls on one side; it cannot support itself long on the forepaws. Mydriasis exists. The temperature falls to 31° C., and the respiration is rendered slower. These symptoms, M. Lombroso thinks, explain the different phenomena of pellagra, and especially of that form to which belong trismus, opisthotonos, paralysis of the legs, with clonic convulsions, and other movements or sensations, as that of a cord passing round the medulla, occur. He has employed the extract in solution in oil, in various inveterate diseases of the skin, as eczema and psoriasis. (*Gazette Médicale de Paris*, December 18th, 1875.)

The Employment of Carlsbad Waters in Diabetes.—Prof. Seegen, in a monograph on the waters of Carlsbad, states that improvement takes place in the general health of all diabetic patients whilst using the Carlsbad waters. In the greater number of cases the excretion of sugar diminishes. It is only in very severe cases that no effect of this kind is observed. Not unfrequently, tolerance of the use of carbohydrates is observed. (*Wien Med. Wochens.*, xiii., 1875.)

Relations of Pustules to Bacteria.—C. Weigert, in a thesis on this subject, observes that in variola (though seldom in the hæmorrhagic form) peculiar miliary bodies (foci) are to be found in the liver, spleen, kidneys, and lymphatic glands, which, when examined, present in their interior a blood-vessel filled with bacteria or a tube of bacteria, the immediate surroundings of which are necrobiotic, as is shown by the absence of nuclei in the cells and other altered conditions. Near these small necrobiotic foci no symptoms of reaction are perceptible in the early stages, though they become very distinct at a later period. The origin of the internal pustules, if they may be so called, is probably similar to those forming the efflorescence on the skin, the differences being essentially due to the nature of the tissue in which they appear—tough skin on the one hand, tender parenchyma on the other. Weigert then proceeds to discuss the diagnosis of bacteria, and after explaining the difficulties of recognising them, especially in the dried mass at a later stage, maintains that the necrosis isa consequence of the accumulation of the bacteria. He is disposed to admit different forms of bacteria, some poisonous, some destitute of any poisonous quality; and he points out the relations between variolous pocks and the membranes in diphtheria and in endocarditis ulcerosa, and the state of the blood in septicæmia. (*Centralblatt f. die Chirurgie*, No. 48, 1875.)

Action of Light on Tissue Change.—The quantity of carbonic acid expired in a given time may be generally taken as a tolerably accurate indication of the rate at which tissue change is going on in the body. Moleschott found that frogs exposed to light expired one-twelfth to one-fourth more carbonic acid than frogs kept in the dark, and the brighter the light was, the more carbonic acid did they excrete. The increased rapidity in tissue change of which this was the indication might of course be due to the action of the sunlight upon the skin, but some recent researches of Von Platen seem to show that it is rather due to the action of light upon the eyes. He kept some rabbits as nearly as possible under the same conditions, but sometimes covered their eyes with black glasses so as to exclude the light, and sometimes with white glasses so as to allow it to act upon them. The experiments which he made in this way showed that when the light was allowed to reach the eye, the carbonic acid excreted was increased by one-sixth to one-seventh of its average amount. Now, a moderate increase in tissue change in a fairly healthy organism leads to increased appetite, increased consumption of food, and a healthier condition of the organism generally, so that anything producing it must rank as a tonic. These researches, therefore, are of extreme interest, as they show what a very high place among tonics must be assigned to light, and the necessity of securing a good supply of it for convalescents and debilitated persons generally. They also tend to explain the weakly condition of the children in the crowded parts of large cities, as well as to some extent the languor which persons not unfrequently feel when they exchange a country for a city life, even although they continue to take active exercise. (*Pflüger's Archiv*, xi. p. 272.)

Use of Pneumatic Pressure in Uterine Displacements.—Professor H. F. Campbell considers that the knee and breast position is most valuable as a means of remedying uterine displacements, and that its occasional failure in some hands is due to the fact that the uterus is held in its abnormal position by pneumatic pressure, and is only restored by means of posture when air is allowed to enter the vagina. For this purpose he recommends the introduction of his "pneumatic repositor," which consists essentially of a glass tube, about two-and-a-half to three inches long, slightly curved at the end, and bulbous to admit of easy introduction. In using it the patient should undress and place herself on the bed on her knees, bending the body forward until the head and thorax rest upon the bed. The face may be turned to one side resting in the two hands, while the elbows are thrown out widely from the sides. The knees are to be separated from five to ten inches. Two precautions must be taken: firstly, the thighs must be perpendicular to the bed, and neither drawn under the belly nor extended backwards; secondly, the patient must arch the spine, not upwards, as that would tighten the abdominal muscles, but downwards, so as to relax them. She then introduces the tube into the vagina for a moment, the air rushes in, and the uterus assumes its proper place. The patient then lies quietly down for the night. The author considers that if this were done every night during pregnancy, not only the bearing-down and other local discomforts attendant upon this condition, but also reflex irritations such as morning sickness, hysterical nervousness, wakefulness, &c., might be diminished and in many cases removed. He also recommends the use of this posture and instrument in the case of virgins suffering from displacements, but averse to any manipulative treatment. He insists that no pessary should ever be applied, or its application attempted, until the dislocated uterus has been reduced by the knee and breast position, assisted by pneumatic pressure. (*Atlanta Medical and Surgical Journal*, May 1875.)

New Source of Lead Poisoning.—M. Lancereaux has discovered a source of saturnine intoxication in the yellow cord-fuses used for lighting cigars. The yellow colour is produced by the chromate of lead in a workshop where the fuses were manufactured. Lancereaux has found that all the workmen had the lead border upon the gums. One of them, whose duty it was to reel the thread, and who absorbed more of the yellow dust than the rest, suffered from lead colic. (*Lyon Médical*, Oct. 3, 1875; *Medical Record*, vol. x., No. 50.)

New Method of Local Depletion.—Dr. Theobald, noticing the inconveniences of the artificial leech, suggests that blood may easily be obtained in moderate quantity from a slight and superficial nick or ablation of a thin disc of skin which has been pinched up with a pair of forceps. He claims for this method that more blood is obtained than by the simple circular artificial leech, that no scar need be left; that it is not so painful, and that it is very simple. With the view of ascertaining what influence some of the substances, which are known to prevent or retard the coagulation of blood, would have in promoting hemorrhage from a wound, such as he has suggested for the artificial leech, he made a number of nicks, as nearly alike as possible, with a sharp razor, into the skin of his fingers, and applied to the different wounds a drop of one of the following solutions, each being of the strength of ʒj to aquæ ℥j: ammon. carb., ammon. muriat., liq. ammon. fort., sodæ bicarb., potas. carb. Besides these he applied to one a weak solution of liq. potassæ in water, to another soap, and to two others he made no application, reserving them for comparison. The influence of the ammonia solutions, more especially of the ammon. carb. and of liq. ammon., in retarding coagulation and promoting the flow of blood, was unmistakable; the others seemed to exert but little influence. Some smarting, of only momentary duration however, followed the applications; and the wounds to which the solutions were applied healed as readily as the others. (*Amer. Journ. of Med. Sci.* No. cxxxix.)

Notes and Queries.

NOTES AND CORRESPONDENCE.

INTERNATIONAL MEDICAL CONGRESS.—We have been requested to inform our readers that the medical societies of Philadelphia, in celebrating the centenary of American Independence, have taken steps to form an International Medical Congress, by appointing a Medical Commission, of which Dr. Gross is to be president, and Drs. Ruschenberger and Stillé vice-presidents. Arrangements have been made for the holding of the congress in the city of Philadelphia, to begin on the 4th and to terminate on the 9th of September, 1876. The Commission propose the following general plan for the organisation and business of the congress:—I. The congress shall consist of delegates, American and foreign, the former representing the American Medical Association and the State and Territorial Medical Societies of the Union; the latter, the principal medical societies of other countries. II. The officers shall consist of a president, ten vice-presidents, four secretaries, a treasurer, and a committee of publication, to be elected by the congress at its first session, on the report of a committee of nomination. III. The morning sessions of the congress shall be devoted to general business and the reading of discourses; the afternoons to the meetings of the sections, of which there shall be nine, viz.:—1, Medicine, including pathology, pathological anatomy, and therapeutics; 2, biology, including anatomy, histology, physiology, and microscopy; 3, surgery; 4, dermatology and syphilology; 5, obstetrics and diseases of women and children; 6, chemistry, toxicology, and medical jurisprudence; 7, sanitary science, including hygiene and medical statistics; 8, ophthalmology and otology; 9, mental diseases. IV. The language of the congress shall be the English, but not to the exclusion of any other language in which members may be able to express themselves more fluently. Gentlemen intending to make communications upon scientific subjects will please notify the Commission at the earliest practicable date, in order that places may be assigned them on the programme. In order to impart to the congress a thoroughly international character, invitations to send delegates will be extended to all the prominent medical societies in Europe, Mexico, the British Dominions, Central and South America, the Sandwich Islands, the East and West Indies, Australia, China, and Japan. Invitations will also be tendered to medical gentlemen of high scientific position; and distinguished visitors may be admitted to membership by a vote of the congress. Among the advantages arising from such a convocation as this, not the least important will be the opportunity afforded its members for the interchange of friendly greetings, the formation of new acquaintances, and the renewal and cementing of old friendships. The Centennial Medical Commission tender in advance to their brethren in all parts of the world a cordial welcome and a generous hospitality during their sojourn in the "Centennial City." The congress will be formally opened at noon, on Monday, the 4th day of September, 1876. The registration-book will be open daily from Thursday, August 31st, from 12 to 3 P.M., in the hall of the College of Physicians, N.E. corner of Thirteenth and Locust Streets. Credentials must in every case be presented. Gentlemen attending the congress can have their correspondence directed to the care of the College of Physicians of Philadelphia, N.E. corner of Locust and Thirteenth Streets, Philadelphia, Pennsylvania. There is every reason to believe that there will be ample hotel accommodation for all strangers visiting Philadelphia in 1876. Further information may be obtained by addressing the corresponding secretaries. The foreign secretaries are—Richard J. Dunglison, 814 N. Sixteenth Street; and R. M. Bertolet, 113 S. Broad Street.

INTERNATIONAL CONGRESS OF MEDICAL SCIENCES AT GENEVA. —This congress, of which Professor Carl Vogt is to be president, will open on the 9th of September, 1877, and will last a week. French is to be the official language. All communications relative either to the congress or to the questions to be discussed at it should be sent to the general secretary, Dr. Prevost, Geneva, on or before June 1st, 1876.

ERRATUM.

THE woodcuts illustrating Dr. LAUDER BRUNTON's paper on "Alcohol" in last number were unfortunately transposed—Fig. 2 for Fig. 1, and *vice versâ*.

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⁂ Any of the foreign works may be procured on application to Messrs. DULAU, of Soho Square, W.C.;—WILLIAMS and NORGATE, of Henrietta Street, Covent Garden, W.C.; or BAILLIÈRE, of King William Street, Charing Cross.

Department of Public Health.

THE RIVERS POLLUTION COMMISSION ON THE DOMESTIC WATER-SUPPLY OF GREAT BRITAIN.

THE sixth and final report of the Rivers Pollution Commissioners has at length been issued. It is a bulky folio volume of 525 pages, with maps and illustrations, and is devoted wholly to the domestic water-supply of the kingdom, both north and south of the Tweed. The Royal Commission appointed to inquire into the best means of preventing the pollution of rivers has had a somewhat eventful history. It was first appointed in 1865, and was originally formed by Mr. Robert Rawlinson, C.E., C.B.; Mr. John Thornhill Harrison, C.E.; and Mr. John Thomas Way, distinguished as a chemist, and especially as an agricultural chemist. This Commission was instructed to inquire how far the present use of rivers or running waters in England, for the purpose of carrying off the sewage of towns and populous places, and the refuse arising from industrial processes and manufactures, can be prevented without risk to the public health or serious injury to such processes and manufactures, and how far such sewage and refuse can be utilised or got rid of otherwise than by discharge into rivers or running waters, or rendered harmless before reaching them; and also for the purpose of inquiring into the effect on the drainage of lands and inhabited places, of obstructions to the natural flow of rivers or streams, caused by mills, weirs, locks, and other navigation works; and into the best means of remedying any evils thence arising. In the instructional letter from the Secretary of State accompanying the warrant appointing the Commission, and addressed to the Commissioners for their guidance, it is observed: "Although it may be taken as proved generally that there is a wide-spread and serious pollution of rivers, both from town sewage and the refuse of mines and manufactories, and that town sewage may be turned to profitable account as a manure, there is not sufficient evidence to show that any measure absolutely prohibiting the discharge of such refuse into rivers, or absolutely compelling town authorities to carry it on the lands, might not be remedying one evil at the cost of an evil still more serious, in the shape of injury to health and damage to manufactures." Again, it observed that, "In the course of these investigations, you [the Commissioners] will make inquiry into the effect on health and comfort of the existing systems of sewage of towns and populous places in the districts examined, and into the best mode of protecting individual and public interests in the purity of running water." Subsequently the Commissioners were instructed to include the question of domestic water-supply in their inquiry. This Commission reported, in three several reports, on the rivers Thames, Lea, and Aire and Calder, when it was dissolved and a new Commission appointed.

The new Commission, appointed in 1868, consisted of Sir William Thomas Denison, K.C.B., Colonel Royal Engineers; Professor Frankland, the distinguished chemist; and Mr. John Chalmers Morton, a scientific agriculturist. This Commission had similar instructions to the preceding Commission, and in a series of reports, the last of which has now appeared, it has dealt in succession with the rivers Mersey and Ribble, with the method of treating sewage known as the A. B. C. process, with pollution arising from the woollen manufacture and the processes connected therewith, with the pollution of the rivers of Scotland, with the pollution arising from mining operations and metal manufactures; and finally, as already stated, with the domestic water-supply of Great Britain.

It is characteristic of the different reports of the two Commissions that the health element of their inquiry, specifically referred to in the instructions given to them, assumes altogether a secondary part in their investigations; and so far as it is dealt with, it is mainly treated in a perfunctory manner. The inquiry, in fact, as was to be anticipated from the constitution of the Commissions, has been conducted principally from the engineering and chemical view of the question, and the public health view, suggested in the instructions, has been very commonly ignored. And yet it may be anticipated that, in the approaching discussion of this subject by the Legislature, this will be considered chiefly in reference to the question put first in the instructions to the Commissioners, whether, in our attempts to remedy one evil, it might not be at the cost of another evil still "more serious, in the shape of injury to health and damage to manufactures." Great as the evils are which arise from the pollution of rivers, so far as it has yet been possible to gauge the sentiments of Parliament and of local authorities in the matter, it is difficult to believe that any general earnest effort will be made for their suppression unless injury to health or to trade, one or both, can be shown conclusively to have arisen from them. The reports of the Commission prove that, so far as trade is concerned, the injury is of a partial character, and not such as to have induced or to show a reasonable probability of hearty co-operation of manufacturers and miners in the objects of the inquiry; and in regard to injury to the public health, the nine voluminous reports of the Commissioners and their ten years' work leave the question precisely where they found it. It might have been supposed that the public health aspect of rivers pollution, obviously, according to the instructions to the Commissioners, held, in the several appointments of the Commissions, to be of at least not less importance than the engineering and chemical aspects, would have been duly provided for in the constitution of the Commission, by including among the Commissioners some one competent to deal with the medical aspects of the question. But such was not the case, and we shall see presently into what a negation this omission has landed the inquiry in its latest work.

The sixth and final report of the Commissioners, chemically and descriptively considered, is a remarkable volume. Never before, probably, was so exhaustive an account of the domestic water-supply of a kingdom penned. On the chemical side it is based upon not less than two thousand analyses; on the descriptive side, upon an examination of the sources of supply of the greater portion of the kingdom on both sides of the Tweed. The general results, we apprehend, as exhibited in the report, will startle even those who are not unfamiliar with the kinds of water supply which obtain over great areas of this country. Roughly speaking, it may be said that half the population of the kingdom have a water-supply for domestic purposes which may be classed with sewage of different degrees of dilution, and that London itself is supplied, for domestic purposes, under the euphonious designation of Thames water, with sewage. A large majority of the cities and large towns, other than London, have at immense cost been able to supply themselves abundantly with palatable and wholesome water; but beyond the limits of these privileged places, water, as a rule, appears to be a mere conventional term used to designate a filthy liquid compound. For all that concerns the details of the various supplies in cities, towns, villages, and palaces, and of the various important chemical questions relating to them, we must refer to the report. We shall touch here only upon the influence of the water-supply upon the production of disease.

The Commissioners treat this question in view of the propagation of epidemics only. At the very outset of their discussion of the question they betray their entrance into an unaccustomed field. They write of the zymotic theory of disease as "belonging to the region of theory," and otherwise loosely refer to "theory" as if it had none but a gossiping connection with pathological science and scientific pathological inquiry. Theory as a necessary means of research does not appear to be acknowledged by the Commissioners in this part of their inquiry. The Commissioners bring together a large number of instances of the propagation of cholera and of typhoid fever by the fouling of drinking-water with the excremental discharges of the sick of those diseases; and upon these instances they found an important argument against the use of water for domestic purposes which is polluted by sewage. But on the other hand they observe (p. 244): "We have shown that the use of water polluted by uninfected sewage—that is, the sewage of persons free from those diseases which are propagated by water—does not seem to be incompatible with the enjoyment of ordinary good health. We know that hundreds of thousands, chiefly of the rural population of the country, habitually consume water strongly impregnated with their own excremental discharges; and we have every reason to believe that even millions of persons in Great Britain are under the same conditions of water-supply, and yet we have no positive evidence to show that such water is *generally* unwholesome; on the contrary, it is consumed from day to day for years, it may be, without producing any marked effect upon the people drinking it." In other words, water contaminated with "ordinary sewage" is, as far as the Commissioners know, harmless to health. Now here is a dilemma. Chemistry, while it fails wholly, as the Commissioners confess, for the detection of the excremental pollutions of water which are mischievous to health, is abundantly efficient for the detection of the pollution arising from ordinary sewage. It helps, in fact, excellently to a knowledge of the conditions of water-supply which are, so far as the Commission knows, not harmful, but not to a knowledge of those which are harmful. The chemistry is in antagonism with the physiological and pathological evidence, yet the Commissioners practically set aside the latter, and base their final conclusions as to domestic water-supply upon the former. Thus they condemn utterly and absolutely the water-supply of London derived from the rivers Thames and Lea, upon the chemical evidence of sewage pollution of the two streams, although they are under the necessity of admitting that they know no harm to health arising from the consumption of Thames water; and the sole instance they cite of harm from Lea water (the instance of the cholera outbreak in East London in 1866) has no bearing upon the question. The pollution of the Lea water at that time arose from the admission into the supply, at a particular period, of foul soakage water from a disused reservoir, and from leakage into a service reservoir, of cesspool matter from a ponded part of the river which served as an outlet for great sewers. Since the removal of the inlets of the great metropolitan water companies to the position they at present occupy on the Thames, no instance of special mischief such as the Commissioners describe is known to have occurred from the water. In fact, the conclusions of the Commissioners as to the use of sewage-polluted water go much beyond the facts they record; and the results of chemical analysis, according to their own showing, are worthless as a test of the wholesomeness or unwholesomeness of the water. It is little likely that the metropolis, or other places similarly circumstanced in regard to their water-supply, will seriously contemplate changes involving enormous cost, so long as the question remains in this position. Inexpressibly disgusting as is the pollution of the Thames and other sewage-fouled sources of water-supply, the "out-of-sight, out-of-mind" principle will prove, we imagine, insensitive to arguments drawn from this chemical point of view. If, indeed, water enriched with ordinary sewage is, as the Commissioners believe, harmless to health, the battle of the waters must be fought out between taste and the rates; and taste, as yet, in this matter, has shown little disposition to come into collision with the ratepayer, and to sacrifice itself as a ratepayer.

But (and here comes the gist of our argument) is it true that the consumption of ordinary sewage in our water-supply is so harmless as the Commissioners represent? Now, they show no evidence in support of this conclusion other than common current belief. The question is one of great complexity and difficulty, and it can only be approached, with hope of successful solution, by men trained to inquiries of the character, backed with resources of a Royal Commission or of a Government Department. It was of the essence of the Commissioners' inquiry in its health aspects that an attempt should be made to solve the question. But the constitution of the Commission forbid such an attempt being made; and thus it happens that Parliament will be called upon to legislate upon matters touching the domestic water-supply of the kingdom, ten years' inquiry by a Royal Commission, on the most important point raised by the inquiry—that relating to the public health—having produced nothing for its guidance but current gossip. Now, the evidence of the harmlessness of water polluted by ordinary sewage, referred to by the Royal Commission, we believe to be worthless. The fact is, that we have no defined scientific knowledge on the subject. But the absence of this knowledge does not prove the absence of mischief. The time has only lately arrived when it has become possible to subject this question to a close examination. Such examination should have been made by the Royal Commission; and if there had been a competent medical member upon it, such an examination, there can be little question, would have been made. There is evidence existing, of which the Commissioners were obviously unaware, which suggests that ordinary sewage mingled with drinking-water is probably a serious source of mischief to the health of many of our populations, shown particularly in the diarrhœal diseases of childhood. The line of inquiry indicated by this evidence is defined, and unquestionably should have been followed by the Commissioners; but it is a line that could not have been pursued successfully without medical co-operation. The work of the Commission in its health aspects will be judged from this point of view, and here it is practically valueless. In other respects, chemical and general, the last of the Commissions' reports is a mine of invaluable knowledge; and we congratulate Professor Frankland, Mr. Chalmers Morton, and Mr. J. R. Smith, the secretary of the Commission, on this worthy termination of their great labours.

In conclusion, we would observe that the Reports of the Rivers Pollution Commission furnish another instance of the too common Governmental craze of attempting to deal with the questions of public health requiring medical knowledge without competent medical assistance. We shall be much mistaken if the omission in this instance does not lead largely to the stultification of the ten years' labours of the Commission.

DR. DE CHAUMONT ON STATE MEDICINE.[1]

THE efforts made by the Society of Apothecaries of late years to maintain the standard of medical education among its members, on a level with the requirements of the day, stand in honourable contrast with the apathy or sluggishness of the Royal College of Physicians, and were never more conspicuous than in the institution of a series of lectures on State Medicine. The

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[1] Lectures on State Medicine delivered before the Society of Apothecaries at their Hall in Blackfriars, May and June, 1875. By F. S. B. François de Chaumont, M.D., F.R.C.S.A., conjoint Professor of Military Hygiene, Army Medical School. London: Smith, Elder, and Co., 1875.

Society has to be congratulated on this further instance of its just appreciation of what the profession have a right to require from an important examining body. It is to be congratulated also on having secured for first lecturer on the subject named Dr. Parkes's colleague at Netley. The selection of Dr. de Chaumont was an earnest of the Society's desire to give fitting dignity to the newly formed lectureship; and the profession at large may now reap the advantage of this choice from the publication of his lectures in a convenient form. The lectures, six in number, are limited to the public health side (so to speak) of State Medicine, and we apprehend that they will prove very welcome to medical officers of health. They present a masterly summary account of the present state of sanitary knowledge and practice in relation to public health administration; just such an account as sanitary workers will care to possess.

It is not unnatural that newly appointed medical officers of health should have displayed a certain fidgetiness in the performance of their duties, and that they should have been disappointed at finding, as a rule, their sanitary authorities rather disposed to resent than otherwise their abounding zeal. Hence there is much grumbling, great disappointment, and not a little disgust among the officers at the indifference and immobility of the powers to which they must defer. The attitude of the medical mind is wholly foreign to do-nothingism. Just as at the bed-side the medical man will despair if he cannot be doing something for the relief of his patient, so in sanitary work he despairs if he cannot see in process of being carried out the measures requisite for the prevention of disease. The newly appointed medical officers of health have come to their districts much with the same state of feeling as they had been accustomed to approach a patient's bed-side, eager to carry into practice what may tend to relief, but too commonly, perhaps, forgetful that, in the case of sanitary authorities, as of patients, there are certain prejudices to be overcome before the authority becomes fully amenable to rule and governance; forgetful also that the prejudices of an individual patient are of trifling moment compared with those possessed by a robust corporate body. The medical officer of health has a long work of education to perform, a work involving much patience and judgment, before he can hope to see removed the difficulties which now interfere with the due exercise of his duties; and his progress for some time to come must be judged by his success in enlisting the attention and securing the confidence of his authority or authorities, rather than by sanitary works actually accomplished. The indifference and apathy which is an indication of want of knowledge is not to be cured by legislation; and it is to be feared that many medical officers of health have weakened their influence with their authorities by persistently urging the necessity of more stringent legislation for the purpose of overcoming the difficulties which beset sanitary work. Legislation which moves too greatly in advance of the knowledge and appreciation of the authorities who have to execute it, must of necessity fail.

Dr. de Chaumont at the outset of his lectures justly touches this question as of cardinal moment in the prosecution of sanitary work. In words which every medical officer of health should weigh carefully, he says:—

"It is a happy augury for the future of our race, that the subject of public health has at last taken a pretty firm hold upon the public mind, although we have still to battle with much opposition, the chief giants in our path being ignorance, vested interests, and the liberty of the subject. The true knight who shall make an end of these is knowledge, led by the hand of time. More extended and compulsory instruction in our schools must be continually urged, so that the habit of understanding biological subjects shall become more or less inherited, and at length, by persistent iteration, questions that appear now to present insuperable difficulties find a ready solution in the most ordinary minds." (p. 2.)

This thoroughly philosophical way of regarding the subject is also the practical way. What is true of our youth is true also of our youthful sanitary authorities. The real results of the work now being carried out by medical officers of health will be gained in the future, and possibly it may be a near future. Dr. de Chaumont tells the story of the progress of sanitary work during the last quarter of a century. Those who have passed through the struggle by which every step of progress was gained, look back with amazement to see how little they had appreciated, during the vexatious disappointments and weariness of the strife, the ground which was gained. This success was won with rudimentary organisations, and, so to speak, rudimentary laws. What then may we not hope for in the near future from organisations and laws approaching more, in several essential particulars, to completeness? We would, therefore, following the example of Mr. Caxton when addressing his promising son Pisistratus, impress upon medical officers of health "the value of patience as regards man and man. You touch there," continued Mr. Caxton, "on the kernel of the social system—the secret that justifies the individual and disciplines the million."

We do not propose to follow Dr. de Chaumont from lecture to lecture. His book is one that it would be to discredit our readers to suppose that they will not familiarise themselves with it. We would, however, especially commend to their attention his observations on statistics in the concluding lecture.

DR. PARKES ON THE PROGRESS OF HYGIENE, 1874—75.

Dr. Parkes's report on hygiene for the year 1874 and part of 1875, recently published in the fifteenth volume of the "Army Medical Department Reports," is, as customary, full of interesting matter. After referring to the great effect produced in practical sanitary work in this country by the appointments made under the Public Health Act 1872, and the more important works on hygiene which have appeared within the period to which his report refers, he proceeds to note the additions made during this period to our hygienic knowledge, which appear to him most worthy of note. Dr. Parkes makes a suggestion in his preliminary observations which deserves attention, namely, that all reports made by medical officers of health to local authorities should be sent to the Local Government Board, and that a summary of them should be made there, after the fashion in which Indian sanitary reports are summarised for the information of the India Office. While on this subject we would recur to a suggestion already made in this journal, the desirableness of Dr. Parkes's several annual reports on hygiene being published in a compendious volume. Few hygienic publications, we are of opinion, would be more generally useful at the present time. As a history of the progress of hygiene during the period of time covered by them, the reports are simply invaluable. How is the War Office to be moved to bring about this needed result? Or, where is the publisher who may be induced to place so valuable a work upon his shelves?

Dr. Parkes discusses in succession (we omit mention of certain subjects treated intercurrently) the additions to our knowledge of the analysis of water and the examination of air, food, and beverages. He then considers the present state of the question relating to the composition of the air in the interstices of the soil, which is not such to admit, as yet, of very definite conclusion. He next touches briefly on the pneumatic (Liernuer's) plan of removing sewage and on disinfection, noting under the latter head the astonishing power of salicylic acid in arresting fermentation. Dr. Parkes then passes to questions concerning the spread of the specific diseases. Here he refers first to typhoid or enteric fever, and notes the now undoubted existence of this disease among the native population of India, mentioning an opinion held by Drs. Hannah and O'Farrell, of the 63rd Regiment, that the late apparent increase of typhoid in India has been simply owing to a more accurate diagnosis. Briefly referring to the more important additions to our knowledge of the conditions under which typhoid is disseminated, derived from the work of the Medical Department of the Local Government Board, Dr. Parkes refers to the vexed question of the autochthonic origin of typhoid, and gives the following history, related by Mr. R. Bond Moore, in reference to it:—

> "At Sedgeley Park School forty-two boys became affected with 'genuine typhoid.' No nurse, no master, servant, or adult of either sex were attacked. Why? They all lived in the same house, drank the same water, ate the same food, cooked in the same way, and several of the masters slept in the same dormitory with the boys. The boys, however, used closets opening into a cesspool emptied in the holidays. These had been used a hundred years and no injury had resulted. A boy one day complained: he had been only four days at the school. His illness turned out to be typhoid. All his evacuations were thrown into the cesspool of the closets which the boys used. All the masters, servants, &c., used closets at a distance. Hence the difference; the boys used closets into the cesspool of which typhoid evacuations were passed, and forty-one, besides the

first case, had typhoid fever. The masters and servants used other closets and did not suffer. The first case had been only four days in the school, and had, therefore, caught the disease elsewhere.

"Two years before a different kind of outbreak had occurred in the same school. This time, masters, servants, and boys were all affected 'indiscriminately.' There was diarrhœa, sickness, great languor, and much prostration. Seventy persons were attacked. The water was found contaminated with sewage from a drain leaking into the well. This was remedied, and the disease ceased forthwith. It was not typhoid fever, but another gastro-intestinal attack. During the next two years the health of the school was satisfactory."

Dr. Parkes gives illustrations of much interest, of the active discussion which still continues in Germany on the influence, as regards typhoid fever, of the ground-water, and of contaminated drinking water. Facts are multiplying in Germany confirmatory of English opinion on these subjects. Further researches into the relation of the movements of the soil-water to the prevalence of enteric fever tend to prove that when such relationship exists it differs widely in its nature in different localities.

With reference to dysentery, Dr. Parkes records an outbreak in barracks at Nurnberg, suggested to have been caused by drinking water which had absorbed putrefaction gases from the cloaca in the wings of the building, acting upon "an individual predisposition." The water contained nitrates and free ammonia.

Cholera is next considered by Dr. Parkes and occupies the remaining portion of his report. After a summary of the conclusions of the International Sanitary Conference which met to consider questions as to this disease, in Vienna in 1874, Dr. Parkes gives an abstract of Professor Hirsch's instructive report on the outbreak and spread of cholera in Posen and Prussia in 1873, so far as it illustrates the effect of human intercourse on the spread of the disease. Professor Hirsch's researches confirm all previous researches, where thorough investigation has been practicable, as to the dependence of the dissemination of the disease upon human intercourse. Dr. Parkes contrasts Dr. Hirsch's conclusions with those of Dr. J. M. Cunningham. He might also very usefully have contrasted the difference in method of investigation as well as of results. The latter are the necessary consequence of the former. We quote Dr. Parkes's abstract of Dr. Hirsch's conclusions from the whole of his inquiry.

"1. *Spread through the traffic of men.*—He says that the fact is confirmed on all sides that the introduction of cholera into Prussian Poland and Prussia was by the Galician raftsmen, and that this was invariably the case. Then from the towns or villages where cholera was thus introduced, and where it spread epidemically in some cases, it spread to places lying far from the river, by inland traffic. 'The introduction,' writes Hirsch, 'was proved with certainty to be either through inhabitants of the place who had been attacked with cholera in other infected places, or through sick persons who had travelled to the place, and everywhere the course of the disease from an individual to his neighbours could be followed.'

"2. *Spread through dead bodies, linen, clothing, and straw.*—The carriage by dead bodies seemed probable, while that by clothing and straw was undeniable. With regard to soiled clothes he mentions the following case:—The clothes of a raftsman, dead of cholera, were stolen and taken to the house of a poor family in the village of Cruczno, the woman of the house and her child sickened with cholera; they were the first cases in the village, and from them proceeded a murderous outbreak. In a village near Culmsee there died all the inhabitants of a house into which linen from a cholera patient, dead at Culmsee, had been brought. After thorough purification of this house, no more cases occurred in that village. Two other similar cases are given.

"With regard to straw, Hirsch speaks with some doubt, but refers to some apparent instances, and thinks it a matter to be carefully looked to.

"3. *Spread through drinking-water.*—Hirsch seems to have no doubt of the influence in spreading cholera of impure drinking-water, but he still thinks it not decided which of the following views is correct, viz.: (1), whether the impure water contains something *specific*; or (2), whether the water merely exerts an injurious influence which *predisposes* to an attack. He relates several instances which certainly seem to give greater weight to the first explanation than the second.

"4. *Spread influenced by ground.*—It so happens that the ground-water during the whole of this year was low; in the Vistula the water-level was extraordinarily low. Hirsch does not, however, conclude much from this. He remarks that in many places the disposition of low-lying parts of towns to be attacked was very marked; there were, however, some exceptions, and he notices two: in Sodgorz, the high-lying places were more attacked than the low-lying, and one of the most severe outbreaks was in a group of houses on an elevation which dominates the whole place. In Elbing the worst outbreak was in a high and dry street with houses built on pure sand.

"5. *Influence of social distress.*—There is no doubt that cholera is very closely influenced by social distress and poverty. Hirsch does not trace out the connection, which, however, in many cases seems easy enough."

Dr. Parkes justly writes of Professor Hirsch that he "is probably the best known writer on epidemics who is now living; his great work (*Handbuch der historisch-geographischen Pathologie*) is a marvel of erudition and of personal investigation; he has paid great attention to the local and favouring conditions which cause wide outbreaks of the specific diseases, and he has shown that he knows how to weigh and judge facts of the most diverse kind."

Dr. Parkes finally gives an abstract of a work[1] by Dr. Frank, who may be styled the Medical Officer of Health for Munich, on the prevalence of cholera in that city in 1873–74. The work is of special interest, as it controverts Prof. Pettenkofer's views with reference to the relation of movements of the sub-soil water there to the quantity of the disease. Dr. Parkes observes:—

> "The most important part of Dr. Frank's book is that in which he deals with the ground-water hypothesis of Pettenkofer. He speaks of Pettenkofer with the greatest respect, but honestly states that the careful measurement of the ground-water gives no support to his favourate theory. He remarks first that the variations in the level of the ground-water, from the year 1856 until now, are too unimportant to be able to account for the putrefactive processes in the ground, and he adduces various reasons for this opinion. Passing, however, to the actual facts of 1873, in January the ground-water was relatively deep; it rose in March, and on the 30th June was higher than it had been for a long time. At that time the cholera commenced: the ground-water fell a little in July, but rose again in August, so that on the 27th of August it stood at its highest point; but while it was thus rising the cholera was at its greatest intensity; from the 27th of August the ground-water fell continuously until November, and during all this time cholera declined, while, according to Pettenkofer's theory, it should have increased; on the 19th of November the ground-water began to rise, and exactly at this time cholera again increased, and reached a second acme. In January the ground-water fell; cholera was less than in December, but still was more than in October and November. In fact, not to go into details, it seems certain that no connection could be maintained between the changes in the ground-water and the cholera cases; and Pettenkofer's hypothesis has thus received a severe and perhaps fatal blow in the very place where the evidence was supposed to be most favourable to it."

Dr. Frank's work contains much other interesting matter.

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[1] "Die Cholera-Epidemie in München, in dem Jahre 1873–74," von Dr. M. Frank. München, 1875.

THE PRACTITIONER.

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Original Communications.

ON THE PROCESS OF FEVER.

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THE following Paper is intended to serve as a compendious statement of the knowledge which at present exists as to the nature of fever. The plan I have adopted in its preparation is in accordance with this intention. It would have been easier to myself, and probably more agreeable to the reader, to have begun by developing in the order of time the views respecting the pathology of fever which have been so warmly discussed in this country and in Germany during the last twenty years, dealing with the facts in connection with the doctrines which have been founded on them. As, however, it appeared to me that this method of treating the subject would not have answered my purpose, I have confined myself, as strictly as I have found it possible, to an exposition of the clinical and experimental observations which relate to the nature of the febrile process, leaving the work of combining them into a theory of fever to a future opportunity. The reader will, however, not find a mere catalogue of dry facts without connection. I have endeavoured to follow such an order in the setting forth of the subjects treated of as appeared to me in accordance with their mutual relations, nor have I hesitated to refer to existing theories, whenever the opportunity has offered itself of pointing out the objections to which each of them is liable.

The recognised characteristics of fever are those which relate either to the disintegration of the living substance of the body or to the increase and diminished constancy of the bodily temperature. But in addition to these attributes which distinguish the febrile state of the human or animal organism from that of health, it is essential to the correct definition of fever that it should comprehend particulars relating to its origin, progress, and termination. For fever is not merely a state, but also a process. It has its beginning in the entrance into or action on the organism of some affecting or infecting cause. After this event follows a period which by analogy with certain physiological processes we are in the habit of calling the period of latency, for it is not until it is passed that the first indications within the affected or infected organism begin to manifest themselves. The state of fever once established, it may vary in its course, in its duration, and in the local inflammations which accompany it, indefinitely; but in all cases it has its onset, accession, and declension, each of which is characterised by more or less distinctive phenomena,—the onset by shivering, accompanied with rise of temperature of the internal parts of the body; the augment by continued pyrexia and exhausting disorder of the bodily functions; the decline, epicrisis, or defervescence by the restoration of those processes one by one to their normal conditions and relations. The period of convalescence which follows decline, during which the mischief done is gradually compensated and repaired, and the injured organism is brought back again to its normal condition (so far as this is possible) by new growth of tissue, does not form a part of the febrile process.

These considerations afford the key to the natural division of the subject and consequently to its methodical treatment. In stating what is known as to the febrile state, I propose to study, first, those facts which relate to the disintegration of the bodily substance, and secondly, those relating to the general disturbance of bodily function of which the disorder of temperature is the chief, reserving for another opportunity the inquiry into the nature of the agents which produce fever, and the mode and limiting conditions of their action.

PART I.—TEXTURAL DISINTEGRATION IN FEVER.

Even to the superficial observer it is obvious that in fever the body wastes. But it is only by the scientific methods of measurement and comparison that we know how this waste takes place—how in a fevered person or animal the living substance of the body is disintegrated. This central attribute or characteristic (as it appears to be) of fever, is known partly from the study of fever artificially induced in the lower animals, partly by the observation of clinical cases. From the former source we obtain facts which are more definite, and thereby better fitted to serve as foundations for our knowledge. But the indications afforded by clinical observation, even though they may be less precise, are often of greater value, because they bring us nearer to the purpose of all pathological inquiry—the learning of the nature and causes of human ailments.

Both in the clinical and in the experimental investigation of diseased processes, measurement without comparison is useless. Thus in health, the discharge of nitrogen in the excreta is exactly counterbalanced by its introduction in food, so that it is only when this is the case that the person or animal can be said to be in a normal state of nutrition, whether the actual quantity of nitrogen which passes through the body in a given time is large or small. If in any disordered condition of health, which is under investigation, it is found that the quantity of nitrogen discharged is absolutely smaller or larger (as the case may be) than in the same individual when well, we can conclude nothing from this, unless it is ascertained at the same time how much nitrogen enters, in order that by balancing the one quantity against the other we may learn whether the amount which is stored in the body is being wasted on the one hand or accumulated on the other. Or, to use the ordinary and perfectly convenient language of physiology, we judge whether integration or disintegration of living substance is going on, not by the absolute income or expenditure, but by the nitrogen exchange—by the excess or defect of nitrogen expenditure as compared with nitrogen income.

Section I.—Experimental Investigations.

For the best observations on the disintegration of tissue in fevered animals we are indebted to Dr. Senator, of Berlin,[1] on the result of whose researches the most important statements contained in this section are founded.

It being desirable to determine not merely the exchange of nitrogen, but also its relations with the production of heat and the discharge of carbonic acid, the method of investigation was contrived and adapted with a view to these objects. As I shall have occasion in the other sections of this paper to refer to it repeatedly, I shall give a sufficiently full explanation of it to enable the reader to understand it.

In order to compare the exchanges of material and of heat in the febrile state with those of health, it was necessary first, to bring the animals to be employed (dogs) into a condition of nutritive equilibrium. With this view each animal was placed for a sufficient time preceding the period of observation, on a diet of carefully-selected horseflesh which was increased or diminished until the body-weight and daily discharge of nitrogen in urea, and the "insensible" loss, became severally constant. After it had been kept on this regulated diet for some days, food was withdrawn for a period of 48 hours, during which the normal discharge of carbonic acid, water, urea, and of heat were measured. The diet was then continued as before until nitrogen equilibrium was once more established. This having been accomplished, fever was induced by the subcutaneous injection of perfectly fresh pus, and determinations of the same kind as had been previously made were repeated *under exactly similar conditions as regards nutrition* during a second period of 48 hours. The febrile condition induced was well characterized. The temperature (as measured in the rectum) began to rise from one to two hours after the injection of the purulent liquid, the pyrexia continuing for about two days. During the accession there were rigors, and the skin felt hot to the finger; subsequently the animals were listless and anorexic, and drank water with avidity whenever it was supplied to them.

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[1] Dr. Senator's contributions to the pathology of fever have been published from time to time during the last ten years in Virchow's Archiv. They have been recently republished under the title of *Untersuchungen über den fieberhaften Process und seine Behandlung*. Berlin, 1873.

Each experiment therefore comprised two 48 hours' periods of observation, separated from each other by an interval of several days, during which periods the production of heat, the changes of bodily weight, the daily quantities of urine and urea excreted, and the respiratory and cutaneous discharges of carbonic acid and water were determined.[1]

In the following table I have brought together the results of a long series of these observations, so far as they relate to the discharge of urea and carbonic acid:—

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[1] The carbonic acid determinations, and the calorimetrical observations, however, related only to limited periods of measurement, each lasting an hour, repeated once or twice during the day. The "insensible loss" was computed for each period by adding the weight of ingesta to the total loss of weight in twenty-four hours, and deducting that of the excreta. As thus reckoned the result for any period of observation would, if the weight of oxygen absorbed were equal to that discharged in the form of carbonic acid, express the discharge of water and carbon from lungs and skin. As however the weight of oxygen taken in in respiration is always in excess of the weight given off, it must be borne in mind that this excess, which is a variable one, is comprised in the so-called "insensible loss." Putting this statement into the equation form :—Insensible loss $= H_2O$ discharge $+ CO_2$ discharge $-$ O absorption.

The collection of the urine in animals is a matter of great difficulty. As regards the dog, the only method that was found to be successful is that of training the animal to empty its bladder at stated intervals. Unfortunately this cannot be done without much trouble. For the continuous collection of the products of respiration and cutaneous exhalation, the animal was placed in a ventilated chamber, which was so arranged as to answer the purpose of a calorimeter. Its construction will be described in the next section. In some instances the carbonic acid was determined volumetrically by Pettenkofer's method, in others by the balance. The water-vapour in the air discharged from the chamber was determined by weight, the apparatus for the purpose being of course interposed between the exit tube and that for the absorption of carbonic acid, so that the air reached the latter in the dry state. In most of the experiments the air as introduced into the chamber was completely saturated, so that, its temperature being known, the percentage per volume of aqueous vapour it contained could be calculated. This being the case, the difference between this percentage and that of the outflowing air gave the quantity of water exhaled by the animal. It was recognised, however, by Dr. Senator that this determination was not exact for much of the out-going water-vapour must have been lost by condensation on the wall of the chamber. Considering that in fever the natural ingestion of food is reduced to a minimum, it is obvious that any comparison made between an animal normally fed, and a fevered animal, would give no exact information. For even if the nitrogen value of the food taken by the animal in the febrile state were accurately determined, no precise estimate of the weight actually assimilated could be founded on it. In health we can do this with precision ; for as soon as the physiologist finds, by the constancy of the weight of the animal, that the income and expenditure of material balance each other, he knows that the expenditure is a true index of the activity of the nutritive process. But in artificial fever, from the short duration and the variableness of the conditions to be investigated, no such balance is attainable ; so that it must always be quite uncertain in the case of an animal which is still digesting a greater or less quantity of food, how much of the nitrogen it discharges is derived from income, and how much from store. In Dr. Senator's experiments the only available method of securing accuracy was adopted. It consisted in observing the same animal after it had been brought to normal equilibrium as above explained, during two successive periods of inanition, separated from each other by an interval, during which the animal was fed as usual, and again brought to nutritive equilibrium.

TABLE I.

SHOWING the RESULTS of QUANTITATIVE DETERMINATIONS of UREA and CARBONIC ANHYDRIDE discharged respectively by a NORMAL ANIMAL, and by the same ANIMAL in the FEBRILE STATE, during successive PERIODS of INANITION.

—			Bodily Temperature in rectum. (Means.)		Discharge of Carbonic Acid in one hour of Observation.		Discharge of Urea in 24 hours.
Experiment 1. Weight of animal, 11 lbs. 10 oz.							
	1st Day	Normal	39·0		3·6		6·01 Grammes
		Fever	39·3		3·4		9·0
	2nd Day	Normal	39·0		3·3		No Observation.
		Fever	40·3		3·7		
Experiment 2. Weight, 13 lbs. 4 oz.							
	1st Day	Normal	39·1		4·03		No Observation.
		Fever	39·4		3·93		
	2nd Day	Normal	39·1		4·78		
		Fever	40·3		4·02		
Experiment 3. Weight, 16 lbs.							
	1st Day	Normal	39·0		2·88		7·89
		Fever	39·6		2·73		14·48
	2nd Day	Normal	38·8		2·72		No Observation.
		Fever	40·7		4·20		
	3rd Day	Fever	40·7		3·77		
Experiment 4. Weight, 10 lbs. 10 oz.							
	1st Day	Normal	39·3		2·72		6·39
		Fever	39·5		2·75		10·27
	2nd Day	Normal	39·3		2·45		No Observation.
		Fever	40·7		2·88		
	3rd Day	Fever	39·6		2·91		
Experiment 5. Weight, 9 lbs. 2 oz.							
	1st Day	Normal	38·9		2·38		6·87
		Fever	39·7	40·3	3·09		11·47
	2nd Day	Normal	38·9		2·20		4·61
		Fever	41·0		2·07		No Observation.
Experiment 6. Weight, 24 lbs.							
	1st Day	Normal	39·0		5·39		10·0
		Fever	39·0		4·38		17·1
	2nd Day	Normal	39·6		5·44		8·1
		Fever	40·0		6·47	6·22	10·91
Experiment 7. Weight, 12 lbs. 3 oz.			Morn.	After.	Morn.	After.	
	1st Day	Normal	38·8	38·8	3·59	4·02	8·86
		Fever	38·7	40·7	2·58	3·26	13·95
	2nd Day	Normal	38·8	38·6	4·09	3·31	7·9
		Fever	40·0	40·0	3·13	3·58	9·47
	3rd Day	Fever	40·4	39·9	3·07	2·79	No Observation.

In this table the approximate weight of the animal to which each experiment refers is stated in the first column. The term "first day" expresses a twenty-four hours period of observation, commencing in each case twenty-four hours after the last feeding. In the same horizontal line with the word "normal" in the second column are set down, first, the temperature of the animal (derived usually from several observations) during the first day of comparative inanition without fever; secondly, its discharge of carbonic acid, measured either during one period of one hour's duration, or during several such periods, as in Observation VII.; and, lastly, its discharge of urea as measured during the whole twenty-four hours. In the second line, opposite the word "fever," the same particulars are registered in the same order as regards the same animal in the same stage of inanition, but in the febrile state. In the two lines following, the Observations relating to the second day of inanition are recorded in a corresponding manner.

The tabular arrangement renders it possible to comprehend at a glance the general bearing of the results. It is seen at once that, although the febrile state as indicated by the temperature, as well as by the general condition of the animal, was thoroughly established, its effect on the rate of discharge of carbonic acid was very inconsiderable. In five, indeed, out of the seven cases, it appeared to exercise no appreciable influence in either direction, and in the other two, although there was during the second day a slight augmentation, it was trifling in amount.

As regards the excretion of urea, the indications afforded by the results are set forth by Dr. Senator to the following effect:[1]—As has been already shown by the investigations of Naunyn,[2] the febrile augmentation of the urea discharge takes place immediately after subcutaneous injection of pus—*i.e.*, at a time which precedes the elevation of temperature. In Observation VII., in which the urine was collected, and its quantity and urea percentage estimated during the first eight hours after injection, it was found that there was already a very marked increase of both, so that, while, as will be seen later, the discharge of heat, of carbonic acid, and of aqueous vapour, may even be diminished during the initial stage of fever, that of urea is augmented. In the later stages this relation is in the main reversed, all discharges, and particularly the "insensible loss," as indicated by the rapid loss of weight, being decidedly increased. In association with this, the urea discharge "remains excessive during the whole course of the fever," while the rate of discharge of urine increases so markedly towards the end of the first day, that, notwithstanding the diminution which is the immediate consequence of the injection, the quantity for the twenty-four hours is in excess.

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[1] Senator, l. c., pp, 59, 60.

[2] *Berliner klinischer Wochenschrift*, 1869, March 15.

Senator further draws attention to the very remarkable fluctuations and inequalities which all the functions investigated exhibit when they are continuously observed. This is seen to be true, not merely of the rate of discharge of urea and carbonic acid, but more strikingly of the loss of weight. When the weighings are repeated at short intervals, it is found that, whereas at one time the loss is scarcely to be estimated, at another it is three or four times as great as that which occurs in health during the same period.[1]

As regards the discharge of carbonic acid, these results go a long way towards negativing the frequently ventured assumption that in fever it is necessarily increased. They have no value, however, as evidences that the carbonic acid discharge is diminished, although, from a number of considerations, it seems highly probable that it is so. For in the experimental comparison of fever with health, it is to be borne in mind that, however exactly you may assimilate the conditions of observation, there is one important condition which, while it appears to belong to the organism, belongs in reality to the environment —viz., that of temperature. The temperature of the body, although it is a direct consequence of the chemical processes of life, may, from another point of view, be quite as correctly regarded as their cause—or at all events, as a modifying condition, for the very increase of heat which fever produces reacts on and influences the fevered body. Consequently, in experiments made on Senator's plan, even when the utmost has been done to render the two observations strictly comparable, the living tissue is not really under identical conditions, for every bit of living protoplasm is acted upon by the general pyrexia, just as if heat were communicated to it from outside. Now, we know that when heat is communicated to the body from surrounding media, so as to elevate its temperature even very slightly, the effect of such slight elevation is to produce a very considerable augmentation of the discharge of carbonic acid, and that this effect is rapidly increased as the temperature communicated to the body approaches that of pyrexia. It is, indeed, quite possible, that if in Senator's observations the comparison had been made between normal and fevered animals at the same bodily temperature, it would have been found that the carbonic acid discharge in the former would have been largely in defect. Comparative observations of this kind have not yet been made, but in the meantime we need have no hesitation in adopting Senator's conclusion, that there is no reason for believing that in induced fever in the dog there is any increased formation of carbonic acid in the body.

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[1] The increase of urea discharged in artificial fever cannot possibly be dependent on the excretion of urea which has previously accumulated in the blood or tissues, for, from the well-established fact that in those tissues which constitute the greater part of the weight of the body, viz., muscles and bones, urea is entirely absent, and that in those organs in which it is most abundant, *e.g.*, the liver, its percentage never exceeds 0·02—0·03, it is clear that even if the whole quantity stored in the body were discharged at once, the effect on the rate of discharge would be barely perceptible. It is therefore obvious that, in the dog at least, its augmentation in the urine is an indication of increased activity of those processes by which it originates in the body, and that the degree of that increased activity may be judged of pretty correctly by the rate of discharge. As, therefore, we are certain that urea derives its nitrogen from albumen, we are also certain that, at all events in artificial fever, albumin of some kind is disintegrated in greater quantity than in health.

From the observations relating to the quantity of urine and from the loss of weight, Senator concludes that the discharge of water is very considerably increased in fevered animals. He attributes the increase in the quantity of urine partly to the presence of an excess of urea in the blood (which, as Ustimovitsch's experiments have shown, acts diuretically), partly to the state of the circulation, and (during the rigor) to the suppression of the exhalation of water from the skin. As the febrile process advanced, the rapid loss of weight (which the carbonic acid determinations during the same period showed not to be attributable to the augmented discharge of that product) indicated a much increased insensible perspiration, so that we must suppose that during the later stages both channels for the discharge of aqueous vapour are wide open. Here, however, as in the case of the carbonic acid discharge, no ground whatever is afforded for the assumption that a greater production of water by oxidation takes place than in health, for there is reason for believing that the direct influence of increased temperature in favouring the exhalation of water is even greater than that which it exercises on the discharge of carbonic acid. On this point again, positive observations are wanting.

The results of Senator's observations relating to the loss of weight in fever and health respectively may be stated as follows:—Fevered dogs lost on the whole from 3 to 4 per cent. of their weight during the first 24 hours of fever. Healthy dogs, in the corresponding period of inanition, lost about 1·6 per cent. As there was certainly no increase of carbonic acid discharge, this must have been chiefly due to loss of water.

Section II.—Clinical Investigations.

It is obvious that the investigation of the exchange of material in fever is much more difficult in man than in the lower animals. This difficulty arises chiefly from the absence of any normal standard with which the condition of the fevered body as regards its income and expenditure can be compared. Just as it was necessary, in the experimental inquiries to which the last section relates, in order to value the results observed in the febrile state to place them side by side with corresponding results relating to animals under precisely similar conditions, fever excepted, so here it is equally plain that if we desire to interpret justly whatever measurements we may be able to make of the physiological products of the human organism under the influence of fever, this can only be done by first ascertaining how much of each product would be yielded by an individual of the same weight under the same circumstances. A patient in fever is in bed and motionless. The food he takes is, as regards the quantity of albuminous material it contains, a mere fraction of his ordinary diet. The first question to be answered in approaching the subject of this section is—What quantities of urea and of carbolic acid ought a normal person in health to discharge from his body when in bed and on fever diet?[1]

The basis of fact on which it is possible to found an estimation of the normal discharge of urea by a healthy individual when on fever diet and in bed, is unfortunately very limited. Indeed, the only observations on the subject which are available are those made by Dr. Unruh[2] on three patients in Professor Leyden's wards at Königsberg in 1869. The patients in question were affected with non-febrile diseases. The first, a cabman aged 47, suffering from carcinoma ventriculi, weighed 104 lbs. His diet consisted of beef-tea or broth, with an egg or two. The quantity was probably excessively small, for the daily discharge of chloride of sodium scarcely exceeded one gramme. He was under observation 19 days, during which his temperature was normal, and the specific gravity of his urine was natural. The mean daily discharge of urea was 17·5 grammes. In two other patients affected with syphilis, each of whom was observed during 13 days, the daily means of urea were severally 18·58 and 16·267, both being on fever diet with milk. Thus we have 17·466 grammes (= 270 grains) as the mean of the three observations; or 0·3835 gramme per kilo. of body weight; whereas the normal excretion of urea per kilo. is about half a gramme. The mean discharge of uric acid in the first case was 0·009 per kilo. per diem.

Having obtained this basis, Dr. Unruh proceeded to investigate in a similar way cases of different kinds of febrile disease, of which the results will be found stated in a long series of tables in his paper. They are summed up by him as follows:—

Assuming that 0·38 grammes per kilo. per diem (= 2·68 grains per lb.) represents sufficiently accurately the normal low diet discharge of urea in health, he found that in one case of relapsing fever, with an evening temperature of 40°·8 C. (105°·4 F.), and a morning temperature of 38°·2 (100°·76 F.) the discharge was on occasion as great as 65·97 grammes, *i.e.* 1·18 per kilo. per diem, more than three times the normal, this being the highest result obtained. The lowest amount in a case of pneumonia (with a history of previous chronic disease) was 18·6 per diem, which is scarcely above the normal. This condition presented itself only during the first few days of observation. The general result of the whole series was that the normal urea discharge per diem in intense fever may be set down at 30·576 grammes (= about 470 grains), hence, something like one and a half times as much as would be discharged by a healthy person on fever diet.

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[1] It is remarkable how frequently this fundamental consideration has been neglected in the discussion of clinical observations on fever. Some clinicists are content with a vague notion that in health something like 500 grains of urea are excreted daily, and that if the febrile discharge is greater than this it must be excessive, and *vice versâ*, forgetting that, unless by some means or other the influence of diet is estimated, the mere determination of the daily discharge of urea is of no value whatever.

[2] Unruh, "Ueber die Stickstoffausscheidung bei fieberhaften Krankheiten:" *Virchow's Archiv.*, vol. xlviii., p. 227.

Dr. Senator thinks this estimate too low, on the ground that in most of the cases the beginning of the febrile process was not included, and consequently that during the period of observation, the patient having been previously subjected for a length of time to the combined influence of inanition and fever, was already exhausted of his store of albumen, so that Dr. Unruh's mean could not be regarded as a fair one; and refers in support of his view to the published urine analyses of Moos and Rosenstein in typhoid and typhus, both of which show that the urea discharge is larger at the very beginning of the attack than it is afterwards. Unfortunately, the number of cases in which the urea discharge has been observed from the beginning to the end is exceedingly small. Dr. Senator has been able to meet with only two, one observed by himself, the other by Dr. Wachsmuth, which were complete in this respect. He has, however, brought together 13 selected cases in which the observations were begun sufficiently early, and in which the significance of the results was not impaired by any conditions affecting the patient. He has divided these 13 cases, which must be regarded as by far the most important body of clinical evidence which exists on the question of the exchange of nitrogen in fever, into categories according to the presence or absence of collateral circumstances which might be supposed to affect the discharge of urea. By doing so, he has been able to estimate the influence of each such condition separately, and thus to add weight to the general conclusions.

One of the most important of these circumstances relates to the previous nutritive condition of the patient. In Voit's well-known observations on inanition, it was found that the time required for the production of a given effect on the exchange of material in any animal varied according to the store of albumen existing in the body, so that, *e.g.*, an ill-fed dog was in the same condition after twenty-four hours as a well-fed after five or six days. The same thing holds good in clinical practice: here, as there, it makes a great deal of difference whether the patient enters into fever with his body well supplied with nutritive materials or not, *i.e.*, whether his previous diet has been abundant or indifferent. The first category includes, therefore, the two cases above referred to, in which it was not only possible to observe the febrile process throughout, but to ascertain the nutritive antecedents with exactitude.

1. Case observed by Dr. Wachsmuth[1]:—Pneumonia in a male, with temperature varying from 40° (104° F.) to 41°·2 (106° F). The patient, who during the first three days took no nourishment, and during the last two days only milk, excreted in the five days, which comprised the whole of the febrile period, 206·44 grammes of urea, *i.e.*, 41·3 grammes daily.

2. Case observed by Dr. Senator in the Charité:—J. H., a male nurse, aged 25, tolerably robust, took ill on the 16th of February, 1870, with relapsing fever. On the following day the temperature had risen to 40°·5 C. (104°·9 F.), the highest point reached. In the course of the 21st, critical defervescence took place, which was accompanied by profuse sweating, lasting over the following day. Up to this time his daily nourishment consisted of from 1,000 to 1,200 cubic centims. (about two pints) of soup, and 400 to 600 cubic centims. of milk. During the six days of the fever, he excreted 240·73 grammes of urea, *i.e.*, 40·1 grammes per diem.

In the case of Dr. Senator's patient, who was a male nurse in the Charité, it was quite certain that the nitrogen value of his previous diet did not correspond to more than about 25 grammes of urea. The other was also an under-fed individual. In each case the febrile condition was over in less than a week; so that the period during which the patients were under observation overlapped that of the duration of fever. The patient was on a diet containing a mere trace of nitrogenous material. The daily quantity of urea as compared with the normal inanition daily discharge above estimated (17 grammes) gives 126 per cent. as the febrile increase.

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[1] Wachsmuth (quoted by Huppert), *Archiv der Heilkunde*, vol. vii.

The second category comprises four cases in which the previous nutrition was similar, but the observation was not begun until two or three days after the accession.

1. Case recorded by Dr. Huppert[1] :—Pneumonia in a male, whose highest temperature was 39°·9 (103°·8 F.) Quantity of urea discharged between the second and seventh day, both included, 200·69 grammes, *i.e.*, 33·5 grammes per diem.

2. Case observed by Pribram and Robitschek[2] :—Relapsing fever in a male, aged 28. Highest temperature recorded 40°·4 C. (104°·72 F.) Between the second and fourth day of his illness he excreted 168·3 grammes of urea, *i.e.*, 56·1 grammes per diem.

3. Case observed by Huppert :—Pneumonia in a male, aged 18, who during the four days following the third of his illness had febrile temperature varying up to 40°·75 C. (105°·3 F.) and took no solid nourishment. The observation was interrupted before the crisis. In the four days he excreted 138·2 grammes of urea, *i.e.*, 34·5 grammes per diem.

4. Case observed by Huppert and Riesell[3] :—Pneumonia in a male, aged 25. During six days of "high fever," from the third to the eighth day of his attack, both included, the total weight of nitrogen in the nourishment consumed by him amounted to 2·97 grammes, while nearly twice as much was discharged by the alvine evacuations and sputa. In the six days he excreted a quantity of nitrogen by the urine, corresponding to 223·6 grammes of urea, *i.e.*, 37·2 grammes of urea per diem.

Mean daily discharge of urea in the four cases, 40·3 grammes.

Here, again, the mean of the results gives about 124 per cent. as the fever increment, showing that the exclusion of the first day or two had no great influence. But in the third category, comprising cases in which the illness was of longer duration, and the observations related to a more advanced period, the mean urea discharge was only 34·5 grammes, and the increment was therefore 98 per cent.

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[1] Huppert, "Ueber die Beziehung der Harnstoffausscheidung zur Körpertemperatur im Fieber." *Arch. der Heilkunde*, vol. vii., p. 1.

[2] For the case of Pribram and Robitschek, Dr. Senator refers to the *Prager Vierteljahrschrift for* 1869.

[3] Huppert und Riesell, "Ueber den Stickstoffumsatz beim Fieber." *Arch. der Heilkunde*, vol. x. 1869, p. 329.

1. Case observed by Unruh (loc. cit. p. 242):—Pneumonia in a male, aged 21, weighing 122 lbs., whose temperature rose during his attack to 40°·5 (104°·9 F.) During the seven days of his illness, beginning with the third and including the crisis, 278·47 grammes of urea were excreted, *i.e.*, 39·8 grammes per diem.

2. Case observed by Huppert (loc. cit. p. 25):—Acute rheumatism with heart complication, in a male aged 21, weighing 130 lbs. Temperature during the first ten days from 38° to 40°. During this period the patient's diet consisted of a variable quantity of milk, with the addition during the first two days of a little soup and bread. On two of the following days no food was taken. The excretion of urea during the ten days amounted to 394·67 grammes, *i.e.*, 39·4 grammes daily.

3. Case observed by Wachsmuth (reported by Huppert, loc. cit.):—Pneumonia in a male, whose highest temperature during the attack was 40°·5 C. (104°·9 F.) Between the second and the eleventh day, during which period the patient took nourishment, he excreted 231·9 grammes of urea, *i.e.*, 23·2 grammes per diem.

4. Case observed by Salkowski:[1]—Erysipelas in a youth of 18, in whom the highest temperature was 40°·2 (104°·4 F.) He was on fever diet from the third to the twelfth day. In eight days he excreted 319·44 grammes of urea, *i.e.*, 39·9 grammes per diem.

5. Case observed by Pribram and Robitschek (loc. cit.):—Relapsing fever in a young man of 22. Highest temperature recorded 40°·4 (104°·7 F.) In the eight days, from the third to the tenth day of the illness, during which the daily diet consisted of soup and a gill of milk, he excreted 218·67 grammes of urea, *i.e.*, 27·3 grammes per diem.

6. Case observed by Senator:—Pneumonia of the right upper lobe in a very robust and well-nourished journeyman slaughterer,

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[1] Salkowski, "Untersuchungen über die Ausscheidung der Alkalisalze." *Virchow's Archiv*, vol. liii. p. 209.

aged 24. The patient, who had shivered on the 2nd of March, was admitted into hospital on the 4th. On the 5th and 6th his temperature reached 40°·5 (104°·9 F.). On the 8th, defervescence took place. During the seven days, from the 3rd to the 9th inclusive, his diet consisted of soup, coffee, and 600 cubic centims. of milk daily; and he excreted 225·10 grammes of urea, *i.e.*, 37·5 per diem.

It is to be noticed that the cases selected were of very various diseases—diseases, in short, which have nothing in common, except the febrile state.

The general conclusion to be derived from the whole series is that in the early stage of fever a patient excretes about three times as much urea as he would do on the same diet if he were in health, the difference between the fevered and the healthy body consisting chiefly in this, that whereas the former discharges a quantity of nitrogen equal to that taken in, the latter wastes the store of nitrogen contained in its own juices. That this disorder of nutrition is an essential constituent of the febrile process is indicated by the fact that it not only accompanies the other phenomena of fever during their whole course, but precedes the earliest symptoms and follows the latest. That it anticipates the beginning of fever was first demonstrated by Dr. Sidney Ringer[1] in his investigation of the relation between temperature and the discharge of urea in ague. That the same condition continues after the crisis has past, *i.e.*, the temperature has begun to sink, was shown by Dr. Squarey in his investigation of eighteen cases of typhus, in all of which the daily excretion of urea was measured, and the variations of temperature were observed during the whole course of the disease, and the observations were continued until convalescence was completely established. In these cases it was found that, whereas the bodily temperature which in this disease rises rapidly at the beginning, and keeps up without sensible abatement during a period which often extends to the middle of the second week, usually begins to fall after the tenth day, the daily rate of discharge of urea, although usually above the normal during the first week, did not attain its maximum until the temperature had been falling for some days.[1]

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[1] On the connection between the heat of the body and the excreted amounts of urea, chloride of sodium, &c., during a fit of ague. *Medico-chirurgical Transactions*, vol. xlii., p. 361. In this paper Dr. Ringer recorded more exact observations than had before been made as to the relation between the temperature and the other phenomena of the paroxysm of ague. In the course of them he made the important discovery referred to in the text, viz., that in this disease the augmentation of the urinary discharge of nitrogen not only begins some time before the accession of the cold stage, but even precedes the rise of temperature.

The question of the source from which the urea increment of fever comes is one which can be better discussed subsequently. At present it is sufficient to notice that the anticipation of the obvious symptoms of illness, particularly of the pyrexia, by the increased excretion of urea, as well as the continuance of the urea excess during the epicritical period, plainly indicate that pyrexia is not the agent by the direct influence of which the increased secretion of urea is produced.

Another consideration suggested by the same facts is this, that the mere increase of the per-centage of urea discharged affords an inadequate measure of the waste of nitrogen, *i.e.* of albumen, which actually occurs in fever; for to form a just estimate, the overlapping at both ends of the process ought clearly to be taken into account. Moreover, in fever there are very frequently losses of nitrogen by the bowels and skin, as well as by exudation, the amount of which scarcely admits of being determined.

It having been established that there is an increased discharge of nitrogen in fever, it remains to state what is known as to its source. There are two sources which are open to discussion, viz.: (1) the albumen of the blood and lymph, and (2) that of the tissues; or, to use the expression which the researches of Voit have rendered current in physiology, store albumen, and tissue albumen. By the former we understand the albuminous constituents of the corpuscles and plasma as well as of the tissue juice or lymph; by the latter, the material of protoplasm, including that of the blood corpuscles.

Here the basis of observation is furnished by researches made by Dr. Salkowski, relating to the proportion of potassium salts discharged by the urine in fever, as compared with that of sodium salts. These researches relate to some twenty cases of various forms of febrile disease in Professor Leyden's wards at

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[1] Observations on the Temperature and the Urine in Typhus Fever, *Medico-chirurgical Transactions*, vol. l., p. 329.

Königsberg. The research began with an investigation of the relative proportion of potassium and sodium salts discharged by the liquid and solid excreta in health, the observer being himself the subject of observation. The diet being mixed, and the nutritive condition nearly that of nitrogen equilibrium as seen by the constancy of the daily discharge of urea (min. 25·3, max. 27·2, mean of seven days 25·69), the daily quantity of potassium and sodium salts respectively, reckoned as potash and soda were: potash, 3·094 grammes, soda, 4·207 grammes; so that of the sum of both alkalies potash constituted 41·4 per cent.

In another individual, a clerk, on low diet without meat, affected with syphilis but in good general health, the soda discharge was about the same, but that of the potash much less, so that the potash per-centage varied from 18 to 26. From these and other observations it was concluded that the daily potash discharge of a healthy person on fever diet is less than one gramme.

The febrile cases investigated were one of relapsing fever, one of erysipelas, and several of pneumonia. In the case of relapsing fever, which was observed during part of the first paroxysm, the whole of the first remission, and of the first relapse and second remission, it was most distinctly seen, that whereas during the remission the potash per-centage of the total discharge of both alkalies sank to about 18·20, it rose during and especially after each crisis to about 90. In the case of erysipelas and in the pneumonia cases there was a corresponding relative and absolute increase of the potash discharge. There were, however, peculiarities in all the cases, which have been fully described by the authors, and are of sufficient importance to require notice.

On the whole, the absolute quantity of potassium discharged on febrile days is three or four times as great as on non-febrile. As regards soda the results are entirely different. During fever it is seen in most of the tables that the soda discharge is extremely low. As soon as the crisis is passed it at once begins to increase to such an extent that in one day as much soda is eliminated as on all the previous days taken together. Simultaneously the per-centage of potash discharge falls to its lowest.

The augmentation of potash discharge in fever, when little or no meat is being taken, and its rapid decline in defervescence, shows that the augmented production of urea in fever must take place at the expense of some source of albumen which contains potash. We have, therefore, in this fact an answer to the question from which we started. The albumen which serves as a source of urea in fever is not derived from liquor sanguinis (for the liquor sanguinis abounds in sodium salts, but contains very little potassium), but either from the blood corpuscles or from muscle, or both.

The very remarkable diminution of the discharge of sodium signifies of course that in fever, the common salt, which constitutes the bulk of the salts of the blood, it retained; for immediately after the crisis (as shown most distinctly in three of the cases), it passed into the urine in great abundance.

In addition to increased excretion of potash there is another circumstance which points to the blood corpuscles or to the muscular tissue as the chief seat of disintegration in fever, namely, the increased discharge of colouring matter. Unfortunately as regards this most important question sufficient information is wanting. There are, to the best of my knowledge, no comparative determinations either of the proportion of blood corpuscles or (what would be as useful) of the iron per-centage of the blood before and after acute fever, either in man or in the lower animals. The only facts relating to the subjects that I know of are, (1) that in all febrile diseases the colouring matter of the urine, which is probably derived ultimately from the blood hæmoglobin, is three or four times as abundant as in health (*see* Neubauer and Vogel); and (2) that after traumatic fever in dogs there is a very marked diminution, both of the corpuscles and of the iron of the blood. But these observations are quite inadequate to serve as a basis for an opinion as to the proportion which the breaking-down of blood corpuscles bears to the total disintegration of fever. Of the many questions which require answering, there is perhaps none which is of greater importance, for if, as appears probable, the destruction of the coloured corpuscles is a part of the febrile process, the fact must have a very important bearing, not merely on the process itself, but on its after results. The colouring matter of the blood being the means by which oxygen is distributed to the tissues, the destruction of it must impair every function of organic life.

As regards the exhalation of carbonic acid in fever, the existing knowledge is much more unsatisfactory than that which relates to the disengagement of nitrogen. Indeed, the only observations on the subject are those of Prof. Leyden.[1] Their very great importance renders it necessary to give an account of them.

Leyden's observations, although perhaps insufficient in number, were made with an exactitude and completeness approaching more nearly to scientific experiment than any other clinical researches with which I am acquainted. Above all, they have the singular merit of being comparative, *i.e.*, that in each case the measurements of carbonic acid discharge in fever were valued, not by a standard derived from averages of observations made on other persons, but by control measurements of the same function in the same individual when in health. Another point of very great importance is that the urea discharge is estimated during the same periods as the carbonic acid discharge, the comparison of the fluctuation of the two functions adding materially to the certainty and value of the results.

The observations were made on four cases, viz., two of relapsing fever, one of typhus, and the other of pneumonia. In the two cases of relapsing fever the quantity of air respired per minute (one of the patients being a male, æt. 63, the other a female, æt. 20) was on the whole 50 per cent. greater than during the interval of apyrexia, the observation being made in the first case before and during the first remission, in the second during the whole of the first remission as well as during the first relapse and the second remission. In the second case the contrast between the febrile and non-febrile days was extremely striking. Thus in the morning of Jan. 23, the last fever day of the first relapse, when the temperature was 40°·1 (104°·2 F.), and the frequency of breathing 34 per minute, the quantity of air respired per minute was 8·13 litres, and the per-centage of carbonic acid contained in it 3·28. In the evening the temperature had fallen to 36°·8 (98°·2 F.), and on the following day (Jan. 24) the temperature was 36°·6, and the rate of breathing 24

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[1] Leyden: "Ueber die Respiration im Fieber," *Deutsches Archiv für klinische Medicin*, vol. vii., 1870, pp. 536–562.

It was then found that the respiration per minute was reduced to 4·85 litres, the per-centage being increased to 3·8.[1]

In the third case, which was one of typhus complicated with hypostatic pneumonia, the most important peculiarity was that during the time that the dyspnœa consequent on the pulmonary affection was most marked, although the temperature had subsided and was nearly normal, the quantity of air breathed was large (10·72 litres per minute), and the per-centage very low. Dr. Leyden has divided the observations relating to this case, which commenced at the maximum of the pyrexia, and were continued through the period of defervescence and the beginning of convalesence, into three parts. In the first, relating to a period during which the temperature ranged from 39°·6 to 40°·1 (103°·3 to 104°·2 F.), the patient breathed 11·7 litres, with a per-centage of carbonic acid of 2·26, the frequency of breathing being 40 per minute; in the second, with a temperature varying from 37° to 39° (98°·6 to 102° F.), (the rate of respiration being still frequent with some dyspnœa), 9·8 litres were breathed, with a per-centage of 1·76; in the third, with perfectly normal temperature and respiration at 22, 6·66 litres, containing 2·9 per cent. of carbonic acid, were expired. Consequently the total discharge of carbonic acid per minute was greater during convalescence than in the epicrisis, notwithstanding the diminished quantity of air respired, in the proportion of 28 to 26. In the fourth case, one of pneumonia, the effect of dyspnœa in increasing the quantity of air breathed is still more marked. Here the observation was commenced at the height of the disease, when the temperature was 40°·5 (104°·9 F.), the pulse 120, and the respirations were 44 to 48. The patient breathed 9·0 litres, containing 3·05 per cent. of carbonic acid. On the next day his temperature was reduced to 39°·1, his pulse to 104 in the morning and to 84 in the evening, his respirations to 36 (morning) and 26 (evening). He respired only 6·8 litres with the same per-centage. During the four following days, when convalescence established itself, the quantity gradually sank to 3·72 litres, while the per-centage increased, but by no means in the same proportion, the highest being 3·45.

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[1] See p. 549 and *seq.*

Dr. Leyden sums up the results of the four cases as follows:—

—	Febrile Period.		Non-febrile Period.	
	Quantity of air respired, in litres, per minute.	Per-centage of Carbonic Acid.	Quantity of air respired, in litres, per minute.	Per-centage of Carbonic Acid.
Case 1	8·76	3·15	6·146	3·4
" 2	7·526	3·79	5·146	3·8
" 3	11·69	2·26	6·66	2·8
" 4	7.912	3·05	4·492	3·2

Hence we have as the general results of his investigation an increase of from 50 to 72 per cent. of the quantity of air breathed per hour, and a diminution of about 10 per cent. in the carbonic acid. Combining these, we have the febrile augmentation of the carbonic acid discharge amounting to nearly 50 per cent.

Before we accept these facts as evidence that increased discharge of carbonic acid is a characteristic of fever, it is necessary here, as with respect to the discharge of nitrogen, to satisfy ourselves that they are not directly consequent on functional disorders, which, although present in the cases observed and often associated with fever, are unquestionably not of its essence. Physiologists are aware that any abnormal elevation of bodily temperature, whether produced by interference with the discharge of heat from the surface or by exposure to an external temperature above the ordinary limits of variation, determines an increased disengagement of carbonic acid. It is also known that this augmentation is associated with increased activity of respiration, but it has not yet been shown whether, on the one hand, the increased liberation of carbonic acid is entirely accounted for by the greater frequency and depth of the respiratory movements, or, on the other, is dependent partly on this cause, but partly also on the increased activity of those chemical processes of which carbonic acid is the product. In other words, it is known that the animal body gives off more carbonic acid when its temperature is slightly elevated above the normal, but it is not yet known whether it produces more.

As regards fever, we are nearly, but not quite, in the same position. In many febrile diseases, no doubt, the increased disengagement of carbonic acid is at first the result of more active respiration, but this explanation cannot apply to any instance in which the fact is observed for more than a few hours. The only way in which increased respiration can produce increased discharge of carbonic acid is by furthering the disengagement of whatever quantity of that body was previously accumulated, an effect which can last only a limited time. We are, therefore, compelled to accept it as a fact that in fever more carbonic acid is actually formed in the body than in health.

We are now in a position to take a general view of the febrile process, so far as relates to the exchange of material and the disintegration of tissue. We have seen that it is established on grounds which do not admit of any question, that a fevered man or animal discharges more nitrogen than a healthy person or animal on the same nitrogen income; and, that as regards man, the febrile excess amounts to something like three quarters of the normal expenditure. We have also seen that in man there is during fever an excess of discharge of carbonic acid, and that this cannot be accounted for as the mere result of excessive respiration, but that no such excess is observed in the dog. We have now to bring these facts into relation with each other.

In health, the whole of the nitrogen discharge is derived from food. In inanition, when nitrogen income vanishes, all the nitrogen which passes out as urea or otherwise is derived from stored or tissue albumen. In fever this is also the case, for the nitrogen income is as defective in the one condition as in the other; but besides the using up of stored albumen, there is an additional and altogether abnormal disintegration which, for reasons already stated, we believe to take place at the expense of blood corpuscles, of muscle or other tissue. That fact must be taken as a starting-point in any attempt to understand the febrile process as a disorder of nutrition.

A healthy adult on ordinary mixed diet discharges from 30 to 36 grammes of urea daily; a healthy adult, on fever diet, discharges from 15 to 20 grammes; but if he is fevered he discharges, on the same diet, a little more than on ordinary diet in health, say from 35 to 40 grammes. A normal person on fever diet discharges about 22 grammes in an hour, of carbonic acid, in health; about 32·3 grammes in fever. Where does this come from?—To account for the urea excess, one must suppose (the nitrogen equivalent of one gramme of urea being three grammes of albumen) that sixty grammes of albumen is disintegrated in 24 hours. These 60 grammes would (the carbon per-centage of albumen being 53) contain 31·8 grammes of carbon, of which one-fifth of the weight of urea discharge (the proportion of carbon in any given weight of urea being $\frac{12}{60}=\frac{1}{5}$,) *i.e.*, four grammes, would pass out as urea. The remainder, viz., 27·8 grammes of carbon correspond to 102 grammes of carbonic acid, which would therefore express the increase of carbonic acid discharge which would result, supposing that all the carbon of the tissue disintegrated to form the febrile excess of urea left the body in that form. 102 grammes per day is only 4¼ grammes per hour; consequently the quantity of carbonic acid produced by the disintegration of albumen in fever is a mere fraction of the total quantity exhaled (32·3 grammes). We have therefore a remainder of 28 grammes which must be derived from the consumption in the body of material not containing nitrogen. The bearing of this fact on the question of febrile thermogenesis will be considered in the next section.

(*To be continued.*)

ON FEBRIFUGES.

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A RATIONAL treatment of fever supposes an exact knowledge of its height and of its general course. We have two physical means for the valuation of both: pulse and internal heat.

Till within late years only the number of pulsations was used as a measure of fever; and often it suffices, as the heart generally beats more rapidly when the blood is heated. Very often, however, this excitement is not the expression of fever, but of mental irritation, poverty of blood, the beginning of a crisis, or of local processes in or near the heart.

The quality of the pulse examined by the finger or the sphygmograph is a still less exact guide to a correct opinion regarding the height of the fever. Tension and relaxation of the arteries may vary to a great degree, whilst the fever remains almost constant.

The chief symptom of the complex condition we call fever is heightened oxidation and disintegration of the living tissues of the body. It follows, therefore, that at present the thermometer is the best means for estimating the height of fever. Measuring warmth with the hand, as some practitioners, relying on habit and expertness, continue to do, is like trusting to the sun for fixing the time. I should say that a well-regulated watch is to be preferred in the one case and a thermometer in the other.

The exact study of the febrifuges has only been rendered possible since the general introduction of the thermometer.

The two chief objects of their employment are: (1), to withdraw a great amount of heat from the burning organism; (2), to lessen the production of warmth.

We attain the first object by directly cooling the surface of the body and through dilatation of the vessels; the second by means of antipyretic drugs.

Let us first take into consideration

The External Application of Cold Water.

It is not long since every fever patient was carefully guarded from pure air and fresh water. Thick blankets and hot beverages seemed indispensable. Several medical men observed that this treatment did more harm than good; but James Currie was the first to have any success in fighting against these prejudices.

Old and deeply-rooted errors do not fall at one blow, especially when they derive support from the extravagances of the opposition. So at least it was with us. Priessnitz, and the fanatic hydrotherapeutists who followed him, barred the way for a long time to the rational use of cool water; and it is only about fifteen years ago that we have recurred to the healthy principles of Currie.

The matter itself is very simple. If a patient at 40° C. (104 F.) is placed in a bath at a lower temperature, he must quickly part with heat. In fever, the natural regulation of heat which keeps our body at an almost equal temperature is insufficient. The cool bath makes up for this. If we measure the temperature after the bath, we shall find it lower than before. The blood that surrounds the cells of our nerve centres is less hot. The patient therefore feels stronger and quieter.

Cold baths (15-20° C. = 60-68 F.) have the clearest effect. Extensive experience has taught that their action is most positive when they are short and often repeated. Very weak patients must begin with 35° (97 F.), and then the warmth must be lowered to 20° (68 F.), by carefully and gradually adding cold water. In the meantime the body should be gently rubbed.

Cold sheets (*Kalte Einwicklungen*) are less efficacious and cold affusions (*Uebergiessungen*) have less effect; this latter considered merely from the antipyretic point of view.

Only a high degree of weakness of the heart, loss of blood or perforation of the bowels, are contra-indications against the use of cool baths. Menstruation is not one when the fever is at a dangerous height, and pregnancy never. Every age and every constitution permits the withdrawal of fever heat, only it must be observed that the loss of heat is in inverse proportion to the weight of the body. For babies we need therefore seldom go under 30° C. (86° F.) to have a full effect; the temperature of the water must be lowest for strong adults.

External application of cold proves, like all other febrifuges, to be most efficacious when the temperature has a tendency to sink spontaneously. That is from seven in the evening till morning, and again in the day from eleven till two o'clock.

The after-effect is of great importance. Under some circumstances it lasts several hours; that is, the lower temperature continues even when the patient has been removed from the water. The reason is probably as follows:—

In fever the vessels of the skin are generally much contracted. The cool water acts as a strong stimulant on them and causes a somewhat stronger contraction to take place, but this is only of short duration. Relaxation for a longer term is the necessary consequence. The hitherto bloodless and dry skin becomes filled and moist, and thus the irradiation of warmth goes on. It is easy to convince oneself of this state of the skin after the bath. The cooler the bath and the longer it lasts, the more evident and the more lasting will be its result.

I only touch on these points, ~~although~~ they are generally known; for the theory of the action of cold water serves partly as a foundation for the following.

Digitalis, Veratria, Aconitia.

In connection with the loss of warmth from the skin, I would mention here some often discussed drugs. Our knowledge of them as febrifuges is still very incomplete, notwithstanding several excellent researches.

If we give moderate doses of digitalis in health, the pressure of the blood in the arterial system rises. Digitalis then acts as tonic on the heart. In what connection does this, its principal pharmaco-dynamic effect, stand to the observation, that digitalis lowers the febrile heat; that often the other febrifuges are efficacious only when they are given together with digitalis? According to my opinion the explanation is this :—

Whenever fever or any other factor of the feverish process has reduced the energy of the left ventricle of the heart, one of the next consequences must be that the arteries will contain too little and the veins too much blood. Especially the large veins of the abdomen are turgid in such cases. It is well known that they are spacious enough to enclose all the blood of the organism. Here the greater part stays, flows languidly to the surface, and a much smaller amount of warmth is given off. Now, every alteration of the pressure in the arterial system must alter the hydrostatic and thermic distribution. In raising this pressure we drive the blood away from the hot interior of the body to the cooler skin. We augment the quickness of the stream, and thus each portion of the blood, in a given time, comes into contact with the external atmosphere in the skin and lungs oftener than before.

No other explanation of the antipyretic effect of digitalis seems to be given as yet, and this may be sufficient. It has at least been proved by experiment[1] that augmenting the pressure in the arteries to a certain degree augments the dispersion of warmth.

On the other hand, digitalis is no trustworthy antipyretic, even when all the said conditions are given. The drug itself changes in strength on a large scale; the single glycosides it contains as active molecules are not yet sufficiently known; it disturbs digestion in almost every case; its wonted effect on the heart comes on slowly and uncertainly; and as soon as you go a little over the line, its *poisonous* qualities begin to act on the heart.[2] Traube, who has used digitalis most extensively in experimental and clinical inquiries, says that its effect on temperature does not take place till thirty-six to sixty hours after its first application, a time which generally does not suit our object.

The true field of action for digitalis is not fever, but that series of heart diseases in which, without fever, the arterial pressure is too low and threatens to produce dropsy.

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[1] Heidenhain in the *Archiv für Physiologie*, iii. 519, 526.

[2] Cf. the most instructive case of W. Murell in this periodical, Nov. 1875, p. 345.

Veratria much resembles digitalis in its general action on the heart. Small doses heighten the pressure of the arterial system. But this is all the light that we have at present on its doubtlessly antipyretic effect. Liebermeister says of it:[1]

"I convinced myself by numerous experiments that a complete intermission can still be achieved in cases where quinine has not sufficient effect. I generally give pills, of which each contains five milligrammes, one every hour till sickness or vomiting ensues. Four to six pills are usually enough. The collapse which often takes place after sickness through the rapidly sinking temperature, is not dangerous, and is quickly relieved by wine or other analeptics. I have only given veratria to patients the action of whose heart was not too weak."

The poisonous effects of veratria have been very clearly described by v. Bezold, but this has as yet produced no result of therapeutic value.

The tincture of veratrum viride is used in America and praised as a most powerful antipyretic. I have no experience of it, either on men or animals. No explanation of its effect, as far as I know, has been given as yet.

Aconitia seems to be a much valued antipyretic in England. Its mode of action is still less known than that of veratria. Its influence on the heart is similar to that of digitalis. The various preparations of aconitia coming from England, Switzerland, or Germany differ very much in chemical quality. This point renders of course all conclusions from experimental researches rather uncertain. Aconite and aconitia are therefore hardly used in Germany. Whether this is wrong or not, can only be proved by further strict experiments.

Alcohol.

I shall now proceed to speak of alcohol, which I regard as occupying an intermediate place between the febrifuge agents previously mentioned, and those which are subsequently to be taken into consideration. It also possesses the property of giving rise to that increased irradiation of heat from the surface of the body which accompanies an accelerated circulation of blood through the cutaneous vessels. It is certain, however, that this is not the only manner in which it acts as an antipyretic.[1]

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[1] *Handbuch der Pathologie und Therapie des Fiebers.* Leipzig, 1875, p. 643.

What we know about the influence exerted by alcohol upon the temperature of warm-blooded animals may be summed up as follows.

To begin with, the increase of animal heat which it was formerly supposed that alcohol causes, does not really take place. The subjective impression of warmth experienced after imbibing a quantity of any strong alcoholic liquor is due, at least partially, to an irritation of the nerves of the stomach, and to the hyperæmia of the skin, where the peripheral nerves receive an impression of increased heat from the caloric passing over in process of being given off. The thermometer—the only reliable guide—indicates no important rise or fall in the temperature of the body after *small* doses of alcohol. Given in quantities a little larger, but still sufficiently moderate as not to cause drunkenness, it causes a distinct fall, lasting half an hour or more, while after a dose powerful enough to inebriate, a still more decided lowering of the temperature—from 3·5 to 5° Fahr.—is observable, which lasts several hours. The decrease of temperature after moderate doses takes place most distinctly in warm-blooded animals to which, for some time previously, no alcohol has been administered. After becoming inured to it, the organism does not respond to such doses by showing any measurable cooling or the reverse.

Well-marked results are yielded more readily by a feverish animal than by a healthy one. For my experiments I generally used strong rabbits or dogs of the same origin and the same quality throughout, and injected into them subcutaneously several cubic centimetres of ichor, or putrefying blood. As is well known, after this proceeding the temperature of the animal rises several degrees, and all the symptoms appear which are to be observed in the case of a human being suffering from putrid fever. The several symptoms referable to the intestinal canal, in particular, remind us strongly of those belonging to enteric or typhoid fever. If the quality of the poisonous substance injected be sufficiently virulent, the animal expires within a few days. Not so, however, if, simultaneously with the commencement of the experiment, alcohol diluted with water be administered, either by means of the hypodermic syringe or by the stomach. The temperature then remains low from the very beginning, the intestinal catarrh is slighter, the animal is more lively, and while the one so treated lives and takes food kindly, another, without the saving influence of the alcohol, may be seen gradually to decline and die. A corresponding result follows if we allow the fever to make a decided progress in both animals before administering the alcohol to one of them.

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[1] I have referred to the principal authors who have written upon this question according to the dates at which their articles have appeared, in the *Journal of Anat. and Physiol.* viii. 242. (London, 1874.)

In such cases, therefore, alcohol shows us a threefold action: (1), it lowers the temperature of the body, or tends to maintain it at a normal temperature despite the influence of an active pyrogenic material in the blood; (2), it resists the process of putrid fermentation; and (3), it stimulates the action of the heart.

Here we clearly see alcohol emerging from the limited sphere allotted to it by many practitioners who look upon it merely as a stimulant. A stimulant for both the heart and nervous system it certainly is, but in the circumstances under consideration it is much more than a simple stimulant.

In these experiments upon animals poisoned with ichor it is clearly to be seen that alcohol, even in powerful doses, need not necessarily act as a narcotic. On the contrary, the ichor-poisoned animal, which at first cowered down in drowsy apathy, after having been dosed with alcohol becomes more lively and begins to run about. Todd and his followers observed similar results in man.

Now, how does alcohol work in the animal economy to bring about the first of the three effects above enumerated, viz., the reduction of temperature?

Reasoning according to popular prejudices, one would think at first that a *rise* of temperature *must* follow the administration of alcohol. Let us, however, consider the matter scientifically. Alcohol becomes readily oxidised, or in other words it *burns*, in the tissues of the body, when it is not given in excessive quantities, forming carbonic acid and water, and during this process of combustion heat is set free.[1] But the same thing occurs, and to a far greater extent, in the assimilation of the numerous oils and fatty substances which we take in such quantities with our food every day, and yet in this instance we are conscious of no *elevation* of temperature, nor does the thermometer show one. The truth is, that the working of our whole system is so admirably ordered, that, despite the development of this heat within the tissues, one almost uniform standard of temperature is maintained, other causes operating to counterbalance the thermic effect generated by the said combustion. I shall not enter here upon a discussion of the various channels through which this compensation may possibly be caused, as I have already explained my views on the subject in the *Journal of Anatomy and Physiology.*

There can be no doubt but that the heart and the skin are two of the media by means of which alcohol acts in causing a change of temperature, the former driving the blood with unwonted energy through the dilated vessels of the latter, where its heat is permitted to irradiate with unusual rapidity; but in my opinion, one of the chief causes for the depression of warmth produced by large doses is a direct impediment offered to the activity of the cells.

The numerous microscopical elements of which our glands are composed, and through whose action the albumen of food is decomposed, become slightly paralysed by alcohol. We have a clear illustration of this fact in the action of alcohol upon yeast, a substance whose cells, endowed with their peculiar catalytic power, may serve here as a type of the more highly organized animal cells. And it may be safely asserted from all points of view, that the cells of the animal organism, on the whole, react toward the paralysing influence of alcohol in the same manner as those of the *mycoderma vini.* Thus the higher the per-centage of alcohol in a fluid with which they are brought in contact, the less able is the protoplasm of the cells to work and produce warmth. And this is not shown in the case of common yeast only, for in every other form of fermentive process the retarding influence in question becomes most apparent, and particularly in that familiar process of oxidation which we call putrefaction. Even the highest and most complicated issue of the protoplasm, the hæmoglobin, is affected by it when in the very act of transferring the oxygen it carries to other substances, the presence of only a very small quantity of alcohol being sufficient to check the rapidity with which this transfer to combustible substances takes place.

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[1] This special point will claim attention further on.

Another series of experiments has proved to me that the *post-mortem temperature* is also susceptible of being lowered by previous injection of alcohol. As is known, this warmth, after the death of almost every warm-blooded animal—particularly when fever has been the cause of death—not only lasts for a longer or shorter time, but even rises by several degrees of the Fahrenheit scale. The fact that this warmth is also under the influence of alcohol, proves to us more strongly than anything else the direct action of that agent on the chemical processes of the animal body.

It has been denied that alcohol so diluted as to be harmless to the body could produce any chemical effect within the circulation. This conclusion, however, is shown to be erroneous by the experiments upon the post-mortem temperature just referred to; for there one certainly has to do with purely chemical processes,—processes depending upon the action of organic ferments, viz., our cells, which, however much they may have depended upon the possession of some peculiar vital property for their activity during life, can give only a chemical reaction after death.

But it requires no special argument to prove that the activity of living cells is, on the whole, much influenced by alcohol when brought in contact with them in quantities too small to be poisonous; and such being the case, we have no grounds for refusing to believe that not only the protoplasm of the nerve-centres, but that protoplasm in general, is similarly acted upon by the same agent.

A priori it is to be expected that alcohol would not be without influence on the metamorphosis of tissues. An agent which, when brought to bear in somewhat larger quantities, so clearly retards combustion, must be supposed to decrease the production of urea and carbonic acid, the two most important excretions of the organism. And, in fact, the researches of several authors prove to us that this is the case.

With regard to the application of alcohol to fever cases, we must not overlook the fact that its effect is not as energetic nor as lasting as that of certain other antipyretic agents, large and repeated doses being required to maintain the lowering of temperature. On the other hand, there are certain cases in which alcohol acts as an antipyretic, whilst quinine is powerless. For instance, Dr. Breisky, now Professor of Obstetrics at Prague, in writing upon this point says:[1]

"A point which I especially emphasise is, that in the fever from resorption of septic material after child-birth, alcohol is a more vigorous antipyretic than quinine. I therefore make use of it as such in the continued fevers also, although I have only been able to obtain decided effects in certain cases which partook somewhat of a remittent form, that is to say, in cases where the production of pyrogenic material was not so abundant as to keep up a constant elevation of temperature."

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[1] F. Conrad, *Ueber Alkohol und Chininbehandlung im Puerperalfieber*. Mit einem Vorwort, von Prof. Breisky. Bern, 1875, p. iv. (Aus den Erfahrungen der Berner geburtshülflichen Klinik.)

(*To be continued.*)

IMPERFECT MASTICATION AS A CAUSE OF DIARRHŒA.

BY ARTHUR W. EDIS, M.D.

IN the able article in your October issue by Dr. Fothergill, on Diarrhœa, he omits to mention one important cause of looseness of the bowels, viz. deficient mastication from defective or decayed teeth. This I have reason to believe from careful observation is a far more frequent cause of irritability of the bowels than is generally supposed. Dentists are familiar with a form of this latter, presumed to be due to the decayed teeth inducing a more or less acid secretion from the gums which produces gastric irritation, and so sets up a species of diarrhœa, or, more properly, looseness of the bowels; but apart from this, I have observed lately, among females especially, a form of looseness and irritability of the bowels, due, I believe, entirely to an inability to masticate properly their food.

Dyspepsia is usually the first symptom noticed, but not by any means invariably. A sudden attack of spasms or abdominal colic, followed by a smart attack of diarrhœa, is frequently the only symptom that will aid us in forming a diagnosis. On inquiry we find this condition is of frequent occurrence, more especially if great care be not taken in the selection of proper food. On examining the mouth, serious defects in the number of the molars will almost invariably be found. Even should they be tolerably sound in one jaw, the teeth that should oppose them are either absent or in such a state of decay that it is painful or impossible to masticate the food properly; this latter is consequently bolted in lumps which, resisting the action of the gastric juice, are carried into the middle intestine and there set up much irritation, increasing the vermicular contraction, as also the secretion of mucus, and so causing a species of diarrhœa.

That this is no imaginary cause I have proved in many instances by suggesting a set of back teeth in place of medicine, and with the effect of obviating entirely the frequent attacks of diarrhœa.

ARREST OF CONVULSIONS BY THE SINISTRO-LATERAL POSTURE.

BY FREDERICK JAMES BROWN, M.D.

I HAVE seen two cases of convulsions arrested almost instantly by turning the patient over upon his left side. I adopted this procedure from experience of the good effects of this posture, both during chloroform inhalation, and subsequently in the stage of recovery from the anæsthetic. The profession owes a debt of gratitude to Mr. Bader for first pointing out the advantages of this posture when danger threatens a patient under chloroform inhalation.

A few months since a man suffering from Bright's disease was seized with uræmic convulsions in my presence. I turned him upon his left side and the convulsions ceased instantly.

Recently a man, aged 56 years, in impaired health from chronic catarrh, was seized with unilateral (right) convulsions. His consciousness and power of speech were intact. He had been convulsed for ten minutes when I entered the house, and he was growing worse. I turned him over upon his left side, and the convulsions ceased in about ten or fifteen seconds. He had experienced a similar seizure on Dec. 9, 1875.

I hasten to report these cases, for I am certain that marvellous results will be obtained in convulsive diseases (possibly even in epilepsy) by sinistro-lateral posture. I conjecture that this form of posture acts beneficially by favouring the action of the heart, but I leave this question to physiologists.

PRACTICAL NOTES UPON GALVANIC INSTRUMENTS AND THEIR APPLICATION IN MEDICINE.

BY HERBERT TIBBITS, M.D., M.R.C.P. EDIN.

Honorary Medical Superintendent of the National Hospital for the Paralysed and Epileptic, and Medical Officer for Electrical Treatment to the Hospital for Sick Children.

THE literature of electro-physiology and therapeutics is abundant, the facts upon which the profession are in accord are well and widely recognised, and the disputed points have received all the light that theoretical dissertations are likely to afford, but yet the *practical use* of electricity makes but slow progress with the bulk of medical men—a fact doubtless explained, to a great extent, by the petty annoyances and difficulties inseparable from the management of the ordinary batteries; for the most profound theoretical knowledge of electro-therapeutics avails little in practice unless the instruments in use are easy and simple in their application, always ready for immediate work, and little likely to get out of order. All *fluid* batteries grow weaker from chemical decomposition and from evaporation, are apt at times to much uncertainty of action, require a good deal of care to maintain their efficiency, and are particularly liable to be rendered unserviceable from spilling of the acid in consequence of being knocked over by a careless attendant, as in transit by a railway porter, an accident damaging also to the carpet or the operator's clothes; while to put them in action they necessitate some arrangement by which their elements are either lowered into the acid, or this is lifted to them, and in either case, if from forgetfulness the plates are left in the acid for a day or a night, they will probably be so damaged that an expense of some pounds will have to be incurred before the battery is again fit for use.

To obviate these disadvantages, I have been experimenting for some years with different elements, with the object of arranging what may be termed for all practical purposes a *dry* battery—a battery particularly suited to the requirements of general practitioners (especially in the country) and for private patients; and recently Mr. Hawksley has successfully constructed for me a Voltaic, a Faradaic, and a Combined battery, free, I venture to think, from the inconveniences mentioned above, and as readily available to the service of the practitioner as his stethoscope or his ophthalmoscope.

(I admit unreservedly that no portable battery can approach, in any medical respect, a large fixed low-tension battery, such as a "*Muirhead-Daniel*," either in regard to its quantity of electricity, its therapeutic result, or the comparative absence of any pain in its application; but it occupies a large space, and as long as patients are not always movable, especially in chronic cases, an efficient portable battery, as facilitating the use of electricity, both in diagnosis and treatment, is absolutely a *sine quâ non.*)

The advantages claimed for these batteries consist, firstly, with regard to their cells, in their remarkable constancy and persistency in action (they remain, with ordinary work and without attention, in full efficiency for quite twelve months and can even then be readily renewed); and secondly, with regard to their coils, interrupting hammers, and accessories, in simplicity of arrangement, solidity, and excellence of workmanship, and non-liability to get out of order. Their greatest advantage is, perhaps, that they are always ready for use, and although, as mentioned below, the sal-ammoniac with which they are charged is in a semi-fluid condition, and they will not admit of *being left turned quite upside down*, they do not suffer from being accidentally overturned if replaced within a few minutes. With ordinary care, indeed, they are essentially dry batteries, and if from gross carelessness there may result any oozing of fluid, it is non-corrosive.

Mr. Hawksley has constructed the Voltaic batteries with any required number of cells from 20 to 100, but the 30-, 40-, and

50-cell batteries are the more generally useful. The elements of the Voltaic batteries have been supplied by the patentees of the Leclanché cell, and consist of a zinc plate inserted in moistened sal-ammoniac, and a carbon plate inserted in powdered peroxide of manganese in a porous cell; the electromotor of the Faradaic coil has been especially constructed with less internal resistance than the ordinary arrangement, while the amount of wire in the coil has been experimentally proportioned to the electromotive power of this element. Fig. 1 represents an ordinary 40-cell Voltaic battery, with graduating dial, and commutator of the poles. A current interrupter has been purposely omitted as an unnecessary complication, as the greater the simplicity of an instrument the greater its utility, and, therapeutically, the best interruption is made by lifting and re-applying one of the electrodes, and if a more rapid interruption is required, the needle of the commutator (which is also a key by which the current may be shut off and on) can be vibrated backwards and forwards.[1]

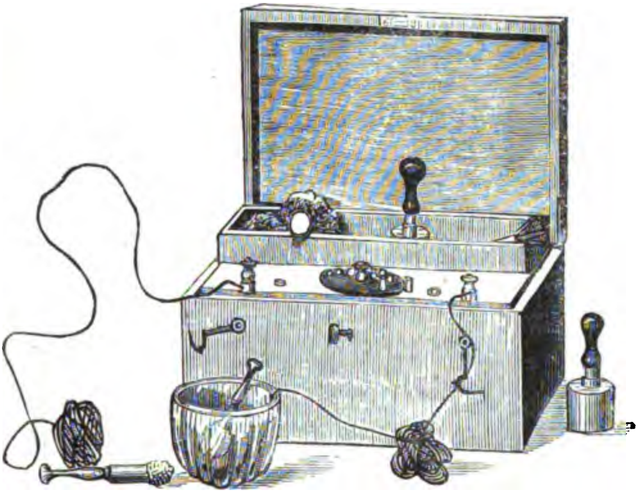

Fig. 1.—Forty-Cell Voltaic Battery.

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[1] The cog-wheel interrupter which was affixed to our battery at the National Hospital some ten years back, has not been used for years, and it is a singular

A bolder innovation has been made in abolishing the primary coil of the Induction instrument (Fig. 2). I have been long satisfied that therapeutically the distinction between the primary and secondary coil consists entirely in the greater tension of the current of the secondary coil, enabling it to penetrate easily several thicknesses of muscle, but there is no therapeutic indication that cannot be fulfilled by the lowest power of this

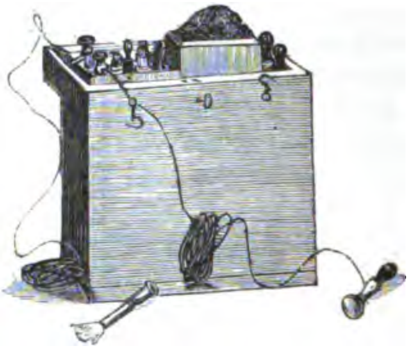

FIG. 2.—Single-Cell Induction Instrument.

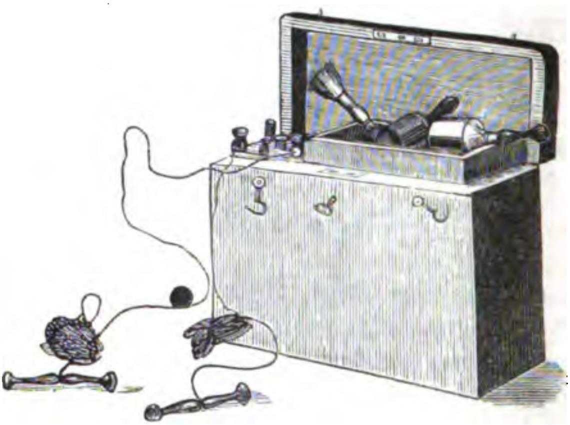

FIG. 3.—Double-Cell Induction Instrument.

instance of the tyranny of surgical instrument makers that they persist, and I have no doubt will persist *ad infinitum*, in putting interrupters to all voltaic batteries.

secondary coil, and I have frequently applied it to the conjunctiva. Fig. 2 represents a single-, and Fig. 3 a double-cell, instrument. Fig. 4 represents a Combined battery, consisting of 40 Voltaic cells and a single-cell Induction instrument, and the conducting-wires are seen to be attached to its two terminals. This arrangement, by which the current of either coil or battery is brought to the same terminals, thus saving the trouble of constantly changing the conductors, though now adopted in other instruments, was first made for me by the late Mr. Becker in 1870. When the needle of the dial, enlarged in Fig. 5, points to "*0*," both currents are quite shut off from the conductors, and this is the position it should occupy when the instrument is not in use. When it covers the stud-lettered "*Coil*," the Faradaic coil is brought into action, and when it points to each number on the dial, the number of Voltaic cells corresponds to such number.

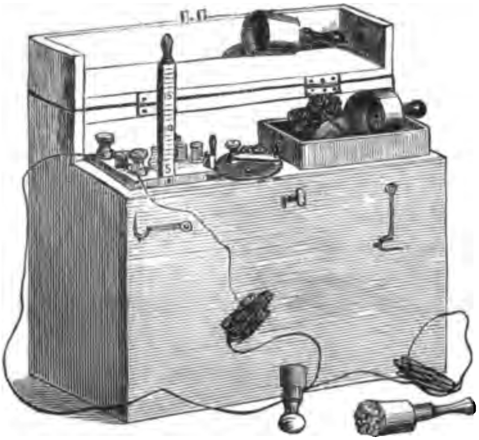

FIG. 4.—Combined Battery.

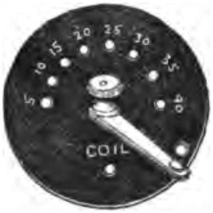

FIG. 5.—Dial of Battery.

The Commutator is a commutator both of the Voltaic and the Faradaic currents. Mr. Hawksley, by packing the cells in tiers, has ingeniously combined the maximum of power with the minimum of space, and the Combined battery is not more bulky than the Voltaic battery alone. The platinum disc against which the needle of the Induction instrument vibrates has been constructed to rotate, and a hole has been drilled in its circumference. By inserting the little lever delineated in Fig. 6 into the hole, the slightest twist given to the disc is sufficient to bring a new surface of platinum into contact with the vibrating needle, and thus the frequent weakening of the current from oxidation of the platinum surface is obviated.

All the batteries described above being in constant action, it is important that the sponge-holders or electrodes should never be left in contact with each other or in the same vessel of water, or fouling and weakening of the battery will result, and care must be taken that the needle of the dial is turned to zero the moment each application is finished. To lessen the risk of the battery being left in action, a stop has been added which shuts off the current when the box is closed, but unfortunately boxes are sometimes left open!

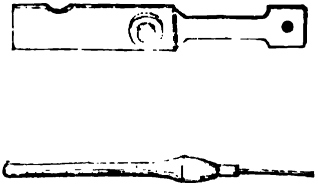

Fig. 6.—Platinum Disc and Lever.

Conducting-cords are always liable to get out of order and cause interruptions in the current, and it is better to use thin copper wire coated with gutta-percha in the same way as that known as " telegraph wire." This is perfectly insulated, sufficiently pliable for all practical purposes, inexpensive, it will fit any sort of conductor, and if its end breaks, all that is necessary is to scrape off with a pocket-knife the coating for about a couple of inches from the broken end. For use when the rheophores remain immovable during the whole time of the application the ordinary telegraph wire is more convenient, and from its much greater diameter will wear much longer, but it is not sufficiently flexible for other purposes. This thicker telegraph wire is especially suited for such applications as that of *positive* Voltaic electricity (originated by Dr. Radcliffe) in which the patient and the battery must both be insulated, in which the passage of the current must be quite constant, and in which a wire, which Dr. Radcliffe terms a "ground wire," must be carried from the negative pole of the battery to the earth. This wire may be conveniently attached to a metallic chandelier or gas-pipe which always gives a direct conduction to the ground. With careful insulation the negative electricity passes away by this wire, and while the current circulates the patient continues "*charged*" with positive electricity. A sheet of gutta-percha about four feet square by half an inch thick (or a piece of floor-cloth) will be sufficient to insulate the patient and the accessories, and it is often convenient in making such an application, to insert the hand or foot of the patient, as the case may be, in a vessel containing tepid salt and water, in which is immersed the wire from one pole, while the sponge from the other pole is held applied to some part of the body, or another limb is immersed with the wire in a second vessel of salt and water. This is essentially the much-vaunted "Electric Bath." For direct muscular electrisation of muscles having a large surface it is most convenient to use well-moistened sponges inserted in cylinders, or metallic discs covered with wet leather, and having conveniently-shaped handles for holding comfortably between the fingers, as in Fig. 7.

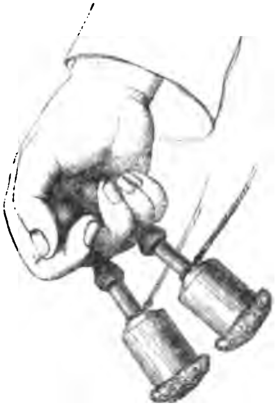

FIG. 7.—Method of holding Rheophores.

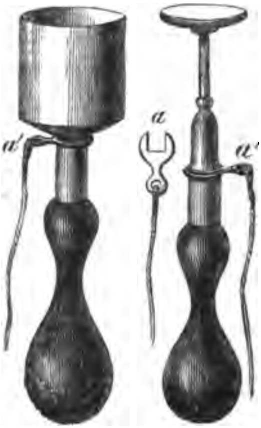

FIG. 8.—Electrodes.

Most sponge-holders are made much too long in the cylinder—

the proper dimensions being about $1\frac{1}{2} \times 1\frac{1}{8}$ inches.[1] The Disc Rheophore shown in Fig. 8, covered with wash-leather, is more generally useful, and by using the edge it may be made to answer in the majority of cases for a pointed conductor. It has also the advantage over the sponge of allowing firm pressure to be made without the inconvenience of water being squeezed out, and Fig. 8 shows the usual method of connecting the conducting-cord with the rheophore, which is seen *in situ* received into the screw-socket of the rheophore in *d.* The cord is very apt to get frayed where it passes through the eyelet-hole, which either entirely stops or causes interruption in the current. That the wire recommended is not open to this objection is not the least of its advantages. The conductors above described are supplied with each instrument, and a wire brush for cutaneous electrisation with the Faradaic and Combined batteries.

I have purposely avoided in this paper any reference to the described conditions in which the use of electricity is indicated. The readers of the *Practitioner* need no instruction upon such a subject, but there is considerable want of information, not so much as to the different modes of applying electricity as to the apparatus by which the applications are best made, and inquiries respecting this are constantly made to the few physicians in this country who have given attention to the subject.[2]

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[1] The sponge should be large enough to fit the cylinder tightly, or it will be apt to slip out, and when not in use it should be taken out of the holder and kept in water until required again. Conductors with fixed sponges, although convenient, soon become fouled, and interrupt the current unless the sponges are frequently renewed.

[2] The wood blocks illustrating this paper have been courteously lent by Messrs. Churchill, having been prepared by them for the forthcoming edition of my *Handbook of Medical Electricity.*

TAPPING THE GALL-BLADDER.

BY E. L. DIXON, L.R.C.P.

In the following case the gall-bladder was tapped five times, not merely with impunity, but with great relief to the patient, and a total of 87½ oz. of liquid was withdrawn. The clinical notes have been condensed within the smallest possible compass.

Ellen Dempsey, aged 55, widow, admitted under my care into Preston Infirmary 14th May, 1875. She enjoyed very good health up to last Christmas, since which she has been steadily losing flesh. This, together with a loss of appetite and gradually increasing weakness, were the first symptoms, as far as could be ascertained from the patient, who, of defective intelligence and education, could not give a very clear account of herself. Her stomach then began to be very irritable, so that vomiting of phlegm was occasionally induced by even so small a quantity as a teaspoonful of liquid food, and she was obliged to give up her work. Jaundice and epigastric pain came on rather suddenly a month before admission, and have continued. The pain, which is almost constant, though occasionally exacerbated, is of a lancinating character, and extends from the region of the liver across the abdomen and downwards. No history of a blow is made out. The liver is slightly enlarged and tender, but the surface is smooth and deep pressure produces a pricking pain. The gall-bladder is evidently much distended, and, though the abdomen is somewhat tympanitic, can be very distinctly felt as a pyriform and elastic tumour, reaching nearly as low as the umbilicus. No other swelling to be made out on palpation; no ascites. No hæmatemesis or hæmorrhoids, but occasionally blood in the constipated evacuations, which are perfectly void of bile. Urine very deeply tinged with bile, and free from albumen. Tongue very foul; emaciation not great; pulse quiet. The temperature was taken night and morning for some time, but was not found above the normal. She said she was of temperate habits, though later, as she grew weaker, she asked to be allowed some whisky.

July 27th.—Notwithstanding the constipation, which has occasionally been relieved with doses of castor-oil, it has been found absolutely necessary, on account of pain, to give opiates, especially at night. Emaciation has increased; the jaundiced hue has become so much intensified that the face is now of an almost olive-green colour (icterus niger), and purpuric spots have made their appearance on the face and arms; evacuations and urine continue unchanged. The liver has enlarged since admission until it now extends to 5½ inches below the ensiform cartilage, and 2½ inches below the ribs on a line with the nipple; its surface is, however, smooth. As a palliative measure, in the hope that she might experience some relief from the severe distension and pain she suffered, a fine needle of the aspirator was passed into the tumour, and by exhaustion 8 oz. of clear, perfectly colourless liquid were withdrawn. She then complained of pain, and the liquid began not to flow so freely; so the cannula was withdrawn, and a fold of lint immediately applied and retained with plaster. The liquid was of sp. gr. 1·004, and became slightly opalescent on boiling and adding nitric acid. Under the microscope abundance of fat globules, a few cells of various sizes, crystals of tyrosine and plates of cholesterine were seen.

August 7th.—No untoward symptoms of any kind, not the slightest tenderness or rise in temperature, were produced by the tapping; on the contrary, the pain from which she suffered was so much relieved that she has been able to sleep without opiates, has taken her food better, and has even sat up. The tongue cleared, and the excessive fœtor of the breath disappeared; the urine and fæces have however remained unchanged. The tumour has again filled; she expresses herself as again suffering very much from it, and is very anxious to be again tapped. The aspirator is therefore used, and 28 oz. of liquid withdrawn, which is of an olive-green colour, faintly albuminous, of sp. gr. 1·004, and giving the play of colours with nitric acid. Under the microscope it presents the same appearance as the liquid withdrawn on the first occasion. It is probably bile.

August 20th.—She was free from pain for some time after the last operation; but now, at her own urgent request, she has again been tapped, and 29 oz. of dark green liquid similar to the last have been withdrawn with great relief.

September 7th.—The aspirator was again used, and 16 oz. of similar liquid withdrawn. The oral mucous membrane is very pale; the tongue is dry, glazed, and fissured; and there is considerable abdominal tympanites.

October 1st.—Rather less tympanites, but some little ascites; no enlargement of the liver to be made out; the gall-bladder feels small and hard. Tapped again at her own request, and 6½ oz. of liquid withdrawn. She takes very little food, and her emaciation and debility have continued to increase, although she has felt very little pain.

She died on the 13th at her own house, whither she had expressed a wish to go.

A *post-mortem* examination (twenty-four hours after death) was obtained with great difficulty, and was made very rapidly under very disagreeable circumstances. Ten pints of ascitic fluid coloured with bile were withdrawn from the abdominal cavity. The stomach was enormously distended with gas. The liver did not extend below the ribs, indeed was less than natural, was of a very dark green colour, soft in texture, and perfectly free from cancerous deposit. The gall-bladder contained 4 oz. of dark green bile similar to what had been drawn off during life, and also two facetted loose gall-stones. Its anterior surface was studded with several small flat warty deposits, the size of large pin-heads, which probably marked the apertures made with the needle of the aspirator; no adhesions to the parietal peritoneum or the surrounding viscera. The hepatic, the cystic, and the adjacent biliary ducts were much dilated, whilst the common duct was occluded by the enlarged head of the pancreas, which was of cartilaginous hardness, and evidently scirrhous. No cancerous deposit was found elsewhere.

The diagnosis of this case was occlusion of the common bile duct, probably from malignant deposit. No free fat was ever observed in the evacuations, which were very frequently examined. The liquid first withdrawn by the aspirator was, as stated, perfectly colourless, and might have been mistaken for hydatid fluid; but it was of too high a specific gravity, and there were no echinococci under the microscope. It was probably bile, from which the colouring matter had been absorbed during its long stay in the gall-bladder. Very little fresh bile must have passed into it for some time, for no alteration in the size of the tumour from the time of admission to that of the first tapping could be made out, whilst on the other hand the liver had, from distension, become considerably enlarged. But after the tapping, bile must have again begun to flow into the gall-bladder, and thus relieve the gorged liver, which then began to diminish in size; for the next time the aspirator was used, the liquid withdrawn presented all the characters of bile. When once the diagnosis was established that the obstruction depended upon an irremovable cause, the operation of tapping could only be considered a temporary expedient; the patient was however much relieved from pain, and her life was probably prolonged by its repeated performance. By withdrawing the accumulated bile at intervals, and thus permitting the flow of the bile from the liver to go on, the pernicious effects upon the liver texture of continued obstruction must certainly have been in some measure mitigated. Acholia, or suppressed secretion of bile, a possible cause of death in such a case, was also thus prevented. The patient indeed died from exhaustion. Her intellect remained serene to the last, and she showed no symptoms of blood-poisoning. The frequent withdrawals of bile with the aspirator were shown by the necropsy to have been perfectly harmlessly effected. In a case of similar obstruction of the gall-bladder, the occasional employment of this instrument, which is moreover comparatively painless in its use, would seem to be preferable to the formation of a biliary fistula by caustic or other means, which has been recommended, but which cannot be considered absolutely free from danger.

DEATH OF DR. WARBURTON BEGBIE.

OUR readers will learn with great regret of the death of this most learned and accomplished physician. When we saw him last, at the meeting of the British Association at Edinburgh, he was feeling far from well and was overburdened with work, but with that kindness of heart and desire to help either his friends individually or the profession collectively, which formed such a prominent feature of his character, he agreed to write a paper for the *Practitioner* on "The Use of Alcohol in Disease," on its being represented to him that he might thus render a service to many of his professional brethren. He was peculiarly fitted to execute such a task, both by his extraordinary powers of accurate observation and his extensive clinical experience, qualities which had already raised him to the foremost place as a consulting physician in Scotland. His aid was sought for far and wide, and such was the confidence in his skill that many a sufferer will mourn his death, not only because one of the best and most generous men that ever lived has departed, but because he feels that he now holds his own life on a more uncertain tenure, and that its term may possibly be shortened by the want of that skill which, in a future illness, might have averted a fatal issue. This confidence was well merited, for Dr. Begbie was not only most accurate in diagnosis, but he possessed a knowledge of remedies and of the best means of applying them, which ensured him success in treatment when others, who could discern the nature of the disease as well as he, would fail to cure it. But while many who knew him only by reputation will grieve over his death, those who knew him personally will feel his loss most bitterly, for to know him was to love him, and the gentle words, the kindly face, and the genial smile, which have ere now gladdened many an aching heart, indicated even to casual observers the noble and generous soul, which further acquaintance enabled them only the better to appreciate. Once a friend he was always a friend, and if there was one above others to whom those who knew him would turn for help and assistance in the hour of distress, that man was Dr. Warburton Begbie. Though he is now gone, his precept and example will long be a living influence in the hearts and minds of those whom he cheered, helped, and taught.

Reviews.

Handbuch der Physiologischen Therapeutik und Materia Medica. Von Professor Dr. HERMAN KÖHLER. 8vo. Pp. 1,336. Göttingen: Vandenhoeck und Ruprecht's Verlag.

Royle's Manual of Materia Medica and Therapeutics. Sixth Edition. Edited by Dr. JOHN HARLEY. Pp. 840. London: Churchill.

Die Hypodermatische Injection der Arzneimittel. Von Professor Dr. ALBERT EULENBURG. Dritte Aufl. Pp. 302. Berlin: Hirschwald.

IN his preface, Dr. Köhler expresses the hope that his work may be useful to those engaged in studying the action of drugs either in the laboratory or at the bedside; and he certainly seems to have spared no pains in order to make it so. It contains a most complete description both of the physiological action and therapeutical uses of nearly all the important drugs at present in use, although cantharides and some other counter-irritants have been omitted, owing to a misadventure. The descriptions are not only complete, but well arranged, so that one can refer at once with the greatest ease to the action of any drug on any organ of the body. But perhaps the most valuable part of the work is the bibliography which it contains. Everyone who has taken the trouble to find out the original source from which almost any statement has been derived knows how much valuable time such a search consumes, and Dr. Köhler certainly deserves the thanks of everyone interested in the action of medicines for sparing them labour and time by giving more copious references to pharmacological literature than almost any other author we know. Stillé's *Therapeutics* and Husemann's *Pflanzenstoffe* approach the present volume most nearly in this respect, but the former is less complete in references to the newest pharmacological work, and the latter deals only with drugs derived from the vegetable kingdom. Altogether the book is one which we can cordially recommend.

Royle's *Manual* has long been prized for the information it afforded regarding the sources and preparation of the various articles in the Pharmacopæia. The accounts of their action and uses given in previous editions were very meagre, but were fully supplemented by Waring's *Manual of Therapeutics*, which formed a companion volume. In the present edition, the pharmaceutical part of the work has been considerably abridged, and the action and uses of drugs described at much greater length. The book is thus rendered much more suitable for students, and its larger page and clearer type make the present much more pleasant to read than former editions. The editor has, we think very unwisely, excluded from the work all the exact knowledge of the action of drugs obtained by experiments on animals, and consequently indulges in speculations where other authors state facts. We would direct his attention to the following passage from Haller, quoted by the Royal Commission on Vivisection: "A single experiment will sometimes refute the laborious speculation of years." If he lays this passage to heart, the next account which he writes of the action of belladonna will be more correct than the present one, in which he attributes the dryness of the mouth produced by the drug to its action on the sympathetic nerves, totally ignoring the fact that Heidenhain has proved it to have an entirely different causation. Sometimes, notwithstanding his determination, he has admitted the results of experiment, as in his description of physostigma, and we trust that in another edition he may altogether prefer facts to fancies.

The hypodermic method of administering remedies, although now so much employed and frequently successful when administration per dram fails, is occasionally made use of with hesitation, because the practitioner feels uncertain regarding the effects of the drug, and the extent to which he may push it in this way. For clearing up any doubts of this sort, Dr. Eulenburg's book is most valuable. In it he discusses the best mode of injection, the best solvents for the remedies, the comparative rapidity of absorption from various parts of the body, the physiological action of all the most important remedies, their therapeutical uses, and the doses in which they may be given. In the present edition he has incorporated the numerous observations which have been made on these points during the eight years which have elapsed since the appearance of the second edition, and notices of several new drugs—such as chloral, apomorphia, and saponin, with cases illustrative of their action—have been introduced, as well as an account of the subcutaneous use of nutritive substances. Notwithstanding all this new matter, the book has been reduced by thirty-six pages by omitting the histories of several cases now no longer required to illustrate the action of the more usual remedies. We are glad to see that the general bibliography has been retained and brought up to date, although the references which were given at the heading of each chapter in the second edition have been omitted. We trust that in a fourth the author will again restore them to their former place.

Clinic of the Month.

Phosphide of Zinc in Mercurial Tremor.—Dr. Noel Gueneau de Mussy observes that phosphorus has, during the last few years, taken a most important place in therapeutics, both as modifying the nervous system and improving its nutrition when impaired. Some years ago M. Vigier pointed out the advantage of phosphide of zinc as a preparation that was much more stable and more easily handled than phosphoric oil, the composition of which was liable to alteration. Four parts of phosphide of zinc contain one part of phosphorus, though the physiological action is equivalent to only half the quantity of phosphorus present. M. Gueneau de Mussy commences with a dose containing about one-sixth of a grain of phosphorus, increasing the quantity gradually, whilst observing the physiological effects, and discontinuing it when any signs of intolerance appear. The following is one of M. Gueneau's cases:—For more than six weeks the patient had been affected with a violent quivering of the limbs which made him quite an invalid. He could not help himself in any way, and even when his arms were resting, lying on the bed, the trembling did not subside. He also complained of pains in his back, and his face was red and flushed. The gums were red, swollen, and bordered with a purple rim. Fearing, from the spinal pains, that the specific poisoning of the nervous system was attended with a congestive process, he did not venture to give phosphide of zinc immediately, but prescribed a purgative, and applied two small cauteries to the base of the neck. After some days, the pain being relieved and the symptoms of irritation somewhat abated, he prescribed two pills of the phosphide, of four milligrammes each. After two days of this treatment, such was the improvement in the patient's health that he could walk alone and eat without being helped, and a few days later he found himself so well, though he was not completely rid of a slight trembling, that he insisted on leaving the hospital. (*Lancet,* February 5, 1876.)

Treatment of Puerperal Convulsions by Non-nitrogenous Diet.—Mr. Churton records several cases of puerperal convulsions, in which the recurrence of the fits appeared to be prevented by this treatment. The following is one of them:—Mrs. W., a small pallid woman in the sixth month of her first pregnancy, was found in a strong fit shortly after dinner. After the fit she was unconscious, her eyes were closed, and nothing could be elicited from her. The fits recurred at short intervals. She was hot, her pulse was quick; she had vomited. She had been out the night before in a cold frosty wind (it also turned out afterwards that she had probably had albuminuria for some months). A large fire was lighted, blankets were thrown over her, all food or drink was warmed before being given; nothing but starchy foods, cream, sugar, and tea (which is said to restrain the waste of tissues and formation of urea), was allowed; no milk, no beef-tea, nor broth. The fits lasted only a very few hours; next day she was perfectly conscious, and was then confined of a living child, which though very small survived a day or two. The first urine that could be obtained was albuminous and had a specific gravity of 1·005. Some months after this, and after prolonged treatment by steel and acetate of ammonia in mixture, the specific gravity was only 1·012 and the albumen persisted. (*British Medical Journal*, March 4, 1876.)

Treatment of Acute Orchitis.—A controversy has been held between Mr. Holmes and Mr. Henry Smith in regard to the propriety of adopting the plan of stabbing the testis in cases of acute orchitis, practised by the latter surgeon for the relief of the pain and cure of the disease, whilst Mr. Holmes denounces it as unnecessary as well as dangerous. Mr. Richardson recommends the following plan as giving rapid and complete relief, the pain in the loins, generally so much complained of, disappearing in about six hours. Two drachms of extract of belladonna to be mixed with half an ounce of glycerine and one ounce of water, and to be laid thickly over the whole scrotum, the testis being supported by a handkerchief applied as a sling. Mr. George Rugg recommends the use of the oleate of mercury with morphia applied to the scrotum. (*Lancet*, February 19, 1876.)

Menière's Disease.—Several cases have recently been reported of this affection. Dr. Duffin brought one case before the Clinical Society of London at the meeting of the 25th February. The patient had suffered from greatly impaired hearing with subjective noises, fits of vertigo, and a tendency to fall to the right. Skull vibrations to a tuning fork were almost, if not quite, absent on the affected side, although the whole range of the pianoforte could be dimly followed. After 18 months considerable amelioration set in, but the left ear then became somewhat involved and the vertigo tended to throw him on his left side. In one of his later fits the patient lost consciousness for the first time. In the subsequent discussion Dr. Hughlings Jackson stated that he had seen several cases, especially in medical men, who often would not believe the ear was affected though troubled with noises in it. He thought hæmorrhage into the labyrinth was sometimes the cause of the symptoms. Dr. Buzzard remarked that he had observed numerous cases in which there was a tripod of symptoms, tinnitus aurium, vertigo, and cranial neuralgia. He thought that in many cases there was no structural lesion of the semicircular canals, the attacks being so short and paroxysmal, and suggested that something like the nerve-storm of migraine swept over the medulla oblongata and produced in the auditory nerve a condition comparable to neuralgia of a common sensory nerve displaying itself by these peculiar symptoms. Dr. Putnam also records a case of Menière's disease in the *Boston Medical and Surgical Journal.* (No. 20, vol. xciii.) In this case the patient had felt almost constant dizziness for 12 years, with noises in the ears like the sound of a storm with rain. The attack came on suddenly accompanied by violent itching of the face. The vertigo was accompanied by apparent movement of objects sometimes from right to left, sometimes from left to right. He suffered occasionally from nausea and vomiting of a greenish or yellowish fluid.

Treatment of Acute Rheumatism by Salicin.—In a paper on this subject by Dr. Maclagan, accompanied by the details of cases, and a chart of temperatures, the following conclusions are given:—Salicin is a valuable remedy in the treatment of acute rheumatism, and the more acute the case the more marked the benefit produced. In acute cases, its beneficial action is generally apparent within twenty-four, and always within forty-eight hours of its administration in sufficient dose. Given thus at the commencement of the attack it seems sometimes to arrest the course of the malady as effectively as quinine cures an ague or ipecacuanha a dysentery. The relief of pain is always one of the earliest effects produced, and in acute cases relief of pain and a fall of temperature generally occur simultaneously. In sub-acute cases the pain is sometimes decidedly relieved before the temperature begins to fall; this is especially the case when, as is frequently observed in those of nervous temperament, the pain is proportionally greater than the abnormal rise of temperature. Finally, in chronic rheumatism salicin sometimes does good where other remedies fail; but it also sometimes fails where others do good. (*Lancet,* March 11, 1876.)

Cæsarian Section successfully performed.—Mr. John Parks, of Bury, Lancashire, records a case in which this operation was performed on a woman aged 32, in labour with the second child, who had been in labour eight hours, and in whom the right foot of the child protruded. On an attempt being made to seize the other foot it was found that the pelvis was too contracted in the antero-posterior diameter to allow of delivery. The circumstances to which Mr. Parks attributes the happy result of the operation are that the woman was of a good sound constitution; that the operation was performed before the powers of nature were greatly exhausted; that great care was taken to keep the abdominal parietes in contact with the uterus during the whole of the operation, thus preventing the exposure of the intestines; that complete closure of the uterus was effected, which prevented the escape of any discharge into the peritoneal cavity; and lastly, that the patient was kept constantly under the influence of opium. (*Lancet*, February 12, 1876.)

Extracts from British and Foreign Journals.

Treatment of Obesity.—Dr. Foot, of the Meath Hospital, gives some clinical observations on a remarkable case of obesity developing suddenly in a boy seventeen years of age. The treatment adopted was so far successful that he was restored from a condition of absolute helplessness to the use of his limbs, was enabled to stand and walk without assistance, and to make himself useful in light work. The medical treatment adopted consisted chiefly in the administration of large doses of liquor potassæ up to ʒij three times a day, and of the liquid extract of the fucus vesiculosus: these remedies were used separately and in combination. The cause of the rather sudden development of adipose tissue about the time of puberty appeared to be connected with an arrest of development of the testis. The age of the lad was 17, his height 4 ft. 5 in., his weight 9 stone 2½ lbs. The fat was chiefly accumulated round the body, neck, and face. The circulation was feeble. The testes were not larger than peas, and the penis did not exceed that of a child a year old. He was very sensitive to cold. The smell of the skin was very offensive from the presence of volatile fatty acids. The potash produced diarrhœa. Notwithstanding the diminution in the amount of fat produced by its use, the patient left the hospital one-and-a-half pounds heavier than on his admission, apparently owing to the formation of muscle. (*Dublin Journal of Medical Science*, No. xlviii.)

Diagnosis between Chronic Diarrhœa and Chronic Dysentery.—A point of distinction between these two diseases is, that the most common time for a patient to be *disturbed* by a diarrhœa is at three or four o'clock in the *morning;* while dysentery is better when the patient is recumbent, and worse when he is moving about, and also soon after taking a meal. (*Medical Record*, No. 261.)

Diagnosis of Syphilis.—Patients will wince when firm pressure is made over the points of junction between the several pieces of the sternum. These points exist even when undue tenderness cannot be detected at any other point in the body, and tenderness also disappears last over these articulations, as the case goes on to recovery. (*Ibid.* No. 261.)

Syphilis as a Cause of Phthisis.—Whether phthisis is ever caused by syphilis has long been a disputed question among physicians. Dr. N. W. Thoreson has investigated the subject very thoroughly, and his memoir obtained the prize of the Medical Society of Christiania.

The history of three hundred and eighteen syphilitic parents is given, as well as that of all their children up to the time of publication of the memoir, which occupies the entire space (124 pp. 8vo.) of the *Norsk Mag. for Laegeridenskaben* for April 1875.

It was often found that a syphilitic mother gave birth to a healthy child, which was nursed by her, and which remained healthy; and, if the mother was not treated, the next child might have marked hereditary syphilis, or present other signs of ill-health. In several instances several healthy children were successively born of such a mother, who then gave birth to syphilitic children. Others had syphilitic children, and then gave birth to perfectly healthy ones. In no case, however, could the origin of phthisis be traced to syphilis in the parents. It was constantly found that, where the children had tubercles, there was a history of this disease in their ancestors. Of the whole number of syphilitic individuals examined, there were only sixteen who had tuberculosis. There were none among these in which the tuberculosis was not found to have existed as a family disease. In the nine tuberculous individuals who became syphilitic the course of the latter disease was very disastrous. The condition of those syphilitic individuals, twelve in number, who belonged to a tuberculous race, but who had not themselves had tubercles, was carefully followed, and, though most of them had severe syphilitic accidents, none of them had any chest-affections, and no other manifestations of tuberculosis were discovered.

The author concludes that syphilis has no causal relation whatever to tuberculosis, though the latter dyscrasia renders the prognosis of syphilis bad, just as any other depressing condition would. He promises to keep all these individuals and their offspring under observation, as far as possible, and to enlighten us further in this direction at some future day. (*New York Medical Journal,* September 1875, p. 320.)

Ancient and Modern Inunction in Health and Disease.—Anointing or inunction, says Dr. E. C. Angell in an article on this subject, is one of the primitive processes of human luxury, invigoration, and refreshment. It was employed at so early an epoch that it is by no means easy to trace its origin in remote antiquity, and it appears to have been in use amongst the most ancient nations. Amongst the Jews the act of anointing was of the simplest character, and consisted in pouring oil upon the head. Amongst the Greeks and Romans the process of what may called hygienic inunction took a much wider range. The anointing of the Greek athletes was doubtless as thorough a rubbing as could well be administered, and its employment by those who made physical perfection the chief aim of their existence is ample proof that it was found beneficial in promoting at once suppleness and strength. It was customary to mix sand with the oil thus used. It was however in the Roman baths of Caracalla, Constantine, Diocletian, and Titus that the art of anointing was carried to its greatest perfection as a means alike of pleasure and of health. A special room, the *unctuarium*, was provided in all the great thermæ for the keeping of the oils and perfumes, the variety of which was very great, including those of cloves, cinnamon, lavender, lilies, myrrh, roses, and many other substances. The anointing process was not confined to those who bathed in a state of health. Galen, describing the treatment of a case of marasmus, explicitly directs that the patient be carried into the tepidarium, when his whole body must be anointed with oil. In recent times the practice of inunction has been strongly recommended by Dr. William Taylor, of London, who, in 1850, published a small volume, entitled "A New and Successful Treatment of Febrile and other Diseases through the Medium of the Cutaneous Surface." In this Dr. Taylor states that during the twelve years from 1837–1849 between 200 and 300 cases of fever occurred at the Clerkenwell Infirmary, to which he was surgeon, without a single death in idiopathic cases; whereas in 1836, before his plan of treatment was adopted, twelve patients died of typhus in one month. He employed lard and suet in equal proportions, and melted them carefully over a slow fire—the combination possessed the consistency of tallow, and was rubbed in night and morning for a period of from half an hour to one hour at a time; "indeed until the skin is saturated, when, from being harsh, hot, and dry, like washed leather, it becomes soft and yields for the most part after a few applications the feeling of velvet. A very large quantity of 'lard ointment' has to be used. Its use extends to all inflammatory, typhoid, and scarlet fevers, measles, and every instance of acute inflammation, where there is much heat, and dryness of the surface. Fever assuming all the typhoid symptoms will be found to change its character completely under this treatment in twenty-four hours. It especially soothes the nervous system, procures sleep, lessens the frequency of the pulse, allays the heat of the skin, and correspondingly the thirst. The pulse may be reduced from 120 to 90 in a few hours after a few applications of the ointment. It corrects the fetid and offensive odour arising from patients; contagion seldom spreads after its use, very rarely even in crowded rooms. When early employed the fever is prevented from running into the continued type, and the patient soon becomes convalescent. Finally, it should be observed that it is always at command, perfectly safe, harmless, and is, perhaps, never contra-indicated." It is useful to strengthen weak lungs, as when bronchitis or pneumonia is threatened, or either has been successfully combated, in which event inunction should be followed up daily, until the case has been conducted through convalescence to complete restoration; and to remove debility of whatever form arising from impaired nutrition. (Under this head marches the great army of dyspeptics, who will find meat and drink, weight and strength, in inunction.) To allay insomnia, hysteria, and incipient insanity; to relieve rheumatism and paralysis; to abort fevers, to guard against contagion.—During the latter part of 1873, Dr. Taylor became acquainted with vaseline, a product of petroleum, which is now pretty well known to the profession and the public, and employs this with advantage as a basis. (*New York Medical Record*, Oct. 1875.)

The Treatment of Erysipelas by the Subcutaneous Injection of Carbolic Acid.—Dr. E. Böckel has tried the plan originally recommended by Hueter of injecting carbolic acid subcutaneously in erysipelas in a series of cases. His method of applying it was to inject morning and evening a certain quantity of solution of carbolic acid of a strength amounting to 1½ per cent., about a third of an inch from the inflamed border, and at several points of the periphery of this part. The erysipetalous inflammation usually overstepped this line during the first few days, but about the third or fourth day its progress was arrested. Dr. Böckel thinks it the most certain means we possess of staying the progress of severe cases of erysipelas. (*Gazette Méd. de Strasburgh*, 1875, No. 5.)

Treatment of Purulent Ophthalmia of New-born Children.—M. Galezowski recommends the following as the most appropriate treatment for this too often neglected, and then serious, affection. He cauterises the eyelids twice a day with a solution of nitrate of silver (1:40 or 1:20) whether the case be a slight or a severe one. From time to time, that is every three or four days, if the conjunctiva is much swollen and presents fleshy folds, he cauterises with the mitigated stick of nitrate of silver (1:3), and if the cornea is hazy he instils a solution of atropine. The nurse or mother should be directed to bathe the lids with warm water every hour or every two hours with a sponge, but never with a syringe. The use of the latter he thinks exposes the attendant to the risk of being affected. In this way, he says, he has treated 600 cases of purulent ophthalmia in ten years, and has not lost a single case, providing the cornea were not already irretrievably damaged. He tells the parents that the disease usually lasts from two to four weeks, that it presents periods of exacerbation, and that the lids sometimes bleed. (*Recueil d'Ophthalmologie*, 2nd ser. Juillet, 1875.)

The Puerperal Diseases of the Mammæ.—At the lying-in institution at Dresden, Dr. Huebner has collected some statistics of the most common affections of the breasts in puerperal women. He states that a previous mastitis does not prevent nursing subsequently, nor does nursing often evoke a new mastitis, although marked alterations in the gland and many cicatrices and contractions predispose to it. His treatment of all lesions of the nipple and areola consists in the constant application, day and night, of lukewarm compresses, wet with lead-water; fissures, ulcers, and excoriations being touched once or twice a day with balsam of Peru, and the breast well supported. The child should nurse less often than usual, and where possible through a nipple-shield. He recommends the warm lead-water in mastitis also, to be followed by strapping of the breast and free incision, while suppuration is promoted by poulticing. (*Deutsche Ztschr. f. prakt. Med.*, 21 and 22, 1875; *Schmidt's Jahrb.* Sept. 8, 1875.)

Treatment of Croup.—Dr. Monti, after giving a good account of the symptoms of croup, states that the employment of alkalies has never been observed to be of service. He takes occasion to notice the fact observed by Dr. Ultzmann that the membrane of croup is rendered fluid in an extraordinarily rapid manner by artificial digestive fluid; such, for instance, as may be obtained from the admixture of one part of pure pepsine with 100 of water, and the addition of ten drops of acetic acid. Nevertheless no experiments have been made with it on the living subject. Emetics he considers are only indicated when the symptoms are those of laryngitis crouposa, as noisy loose cough, rattling respiration, &c.; when these symptoms are absent emetics should be avoided. (This however is opposed to the views and practice of many eminent authorities.) He strongly advocates tracheotomy, which should be performed early, as in the advanced second stage, or before the symptoms of asphyxia have become too strongly marked. Nothing is gained by delay, whilst the results of the operation are interfered with to a serious extent. It should however never be omitted. The choice of the cannula is a matter of considerable importance, and its size should be in accurate relation to that of the tracheal wound in order to avoid emphysema, which, if extensive, is a very serious complication. After the operation, Monti gives large doses (thirty grains) of tannate of quinine. (*Wiener Klinik,* i.)

The Abortive Treatment of Dysentery by Salts and Chloral.—Dr. David Prince, of Jacksonville, Ills., gives the following definition of dysentery:—A disease of the large intestine, characterised by tormina, tenesmus, and mucous and bloody stools; the pathology consisting in congestion, in the disturbance of vermicular contraction, and in the development of an inflammation whose seat is by choice in the rectum, and which, like erysipelas, tends to enlarge its area. The indications for treatment are—1. To produce diarrhœa by agents which do not themselves excite mucous inflammation. 2. To counteract or suppress the specific inflammation. 3. To secure rest both of the body generally and of the alimentary canal in particular. For the fulfilment of the first indication the neutral salts are especially adapted, since they produce a watery discharge capable of relieving congestion and unloading the hæmorrhoidal vessels. The sulphate of magnesia is the best; but tartrate of potash and soda is in much esteem. The addition of senna and rhubarb is very common. To subdue the inflammation—assuming that the disease spreads along the mucous membrane as erysipelas spreads over the skin—oil of turpentine and oil of cinnamon have been employed in connection with castor oil as a cathartic in the early stages, and in the later stages castor oil has been given with opium in the form of an emulsion to quiet irritation. Of late, however, the hydrate of chloral has been found to exert an extremely beneficial action. As an abortive of dysentery it is given in a dose large enough (thirty grains) to produce sleep, in conjunction with one or two ounces of sulphate of magnesia. Its effect may be aided by a subcutaneous injection of morphia. (*Saint Louis Med. and Surg. Journ.,* No. 9, 1876.)

Aphonia, its Causes and Treatment.—Dr. Porter considers the causes of aphonia as affecting one of three factors in the formation of voice. Those impairing the first element—the supply of air—are phthisis, debility, and tracheotomy. Those that alter the condition of the second—the outline or elasticity of the vocal cords—are divided into local and constitutional affections. The former are acute and chronic inflammations and morbid growths; the latter, phthisis, syphilis, and the exanthemata. Of the third and last class—those which impede the action of the muscles moving the cords—especially the adductors and tensors—the principal members are mechanical obstrucction, bilateral and unilateral paralysis of these muscles, and possibly a reflex nerve action from irritation elsewhere. (*St. Louis Med. and Surg. Journ.*, Jan. and Feb., 1876.)

Topical Blood-letting.—Dr. Mapother observes that it is comparatively useless to bleed, as is often done, from veins having little or no direct connection with an inflamed or congested part, and thus recapitulates some of the organs and the superficial spots whence they may be drained:—The eye at the mastoid process and angle of jaw; the cerebral hemispheres at the nasal septum and posterior inferior angle of the parietal; the base of brain and the ear at the mastoid; the right heart over the thyroid body; the pericardium and front of pleuræ outside the edges of sternum; the lungs along the bases of scapulæ and the digestive organs from the rectum. From the veins of this gut also, those of the bladder and prostate, uterus and ovaries, can be depleted. (*Dublin Journ. of Med. Science*, March.)

Treatment of Typhoid Fever.—Dr. M. H. Schooley, at the conclusion of a long article on typhoid fever, remarks that great individual difference of opinion exists in regard to its treatment. Some prescribe milk and water, others a hydropathic course, whilst Liebermeister gives calomel in doses of seven grains three or four times in the twenty-four hours, believing that it materially shortens the duration of the disease and diminishes its intensity. With this plan Dr. Schooley fully coincides. He commences, if called in moderately early at the commencement of second week with a brisk purge (calomel gr. x pulv. rhei. gr. v.) and then begins with calomel gr. ij., pulv. Doveri. gr. vj., pulv. plumbi. acet. gr. j. This is repeated every four or six hours. If there is a tendency to diarrhœa he substitutes one grain or more of opium for Dover's powder, with some increase of the lead acetate. He alternates the powder with from six to eight drops of turpentine. An approximation to an amelioration of symptoms is usually manifested on the afternoon of the sixth day, sometimes before; and at 6 P.M. of this day he suspends former treatment and administers castor oil ℥j, and turpentine gtt. x, expecting its cathartic effect between 10 and 12 P.M. Should the resulting dejections be of a tarlike consistence and colour, he confidently expects the patient to pass rapidly into convalescence, especially if the tongue remain moist and the buccal secretions are free. Then as a supporting and reparative measure he prescribes quinine (gr. jv.) every two hours, until six such doses are taken, beginning immediately after midnight or as soon as the bowels are relieved. He never prescribes quinine at any other time, and especially avoids giving it with a *dry* or a *red dry tongue*, as it then acts as an irritative. If, after the use of the castor oil and turpentine the dejections are then light-coloured and perhaps flaky, the disease will not terminate on the seventh day. Throughout the second week the patient should be well supported with beef-tea, gruel highly seasoned with salt, until the approach of the thirteenth day, when the cathartic, quinine, &c. must be repeated as on the sixth day. (*Saint Louis Med. and Surg. Journ.*, No. 11, 1875.)

The Use of Hot Water as a Permanent Bath in Injuries.—Hot water has, within the past few months, been used in the treatment of some injuries, with marvellous results, in the Bellevue Hospital, New York. The manner of using it is to have tin vessels, formed so as to accommodate the extremities, and to them fastenings are attached, by which bandages can be applied, so as to steady the limb in any position. The water in the bath varies from 100° to 105° Fahr., and is changed as soon as it falls below this. An additional advantage is obtained by the change of the water, as any discharge which forms is removed. The following cases have been successfully treated:—Compound comminuted fracture of metacarpal bones; compound dislocation of the ankle-joint; compound fracture of the metatarsal bones; gunshot injury of the hand; contusion of toe. The time that the injured part is allowed to remain in the hot-water bath varies. The rule is, that it should remain there till all sloughs which form come away, and leave a healthy granulating surface. The time varies with the amount of the injury, but is usually from one to three weeks. (*New York Med. Journ.* xxii., p. 158.)

Prevention of the Formation of Milk.—At a meeting of the New York Academy of Medicine, Dr. Peaslee said that he had received from Dr. W. R. Wilson, U.S. Army, stationed at Fort Wayne, a communication in regard to the treatment of the breasts in cases where it was desirable to prevent lactation. Dr. Wilson wished him to get the opinion of the Academy on the subject. Dr. Wilson's method consisted in strapping the breasts tightly after delivery, by means of strips of adhesive plaster. Dr. Peaslee said, since his attention had been directed to the matter by Dr. Wilson, he had tried in five cases of still-births, where it was desirable to suppress the milk. In all of the five cases the results were perfect. The method of using extract of belladonna had never been satisfactory, and the suggestion of Dr. Wilson was very important. (*Ibid.* p. 167.)

Notes and Queries.

THE UNIVERSAL DISINFECTING POWDER.—This disinfecting powder, made by Ledger and Co., Lant Street, Borough, unites several properties which had previously been required and had not yet been combined in any one substance. Formed of a combination of zinc sulphate with calcium and sodium chloride, it forms, in action, a chloride of zinc with sulphates and chlorides of sodium and calcium. This solution is eminently disinfectant; and may be used for cesspools and sewers, for destroying the infection of the discharges in the sick chamber and hospital, and answering all the purposes of a cheap and efficient disinfectant and deodorizer for daily use in houses and public places, and for urban as well as rural sanitation. Being much cheaper than carbolic acid, the powder claims to be employed by medical officers and inspectors of nuisances, managers of hospitals, and medical men generally. We commend it to their attention.

PRESCRIPTION WINES.—Not only are the bad effects of excessive indulgence in alcoholic beverages greatly increased, but their good effects in disease may be very seriously diminished, and even, perhaps, completely counteracted, by the deleterious substances with which wines and spirits are adulterated. It is, therefore, important for the ordinary consumer, and still more so for the physician, to employ spirits and wines on the purity of which he can rely, but it is exceedingly difficult for him to choose between the various spirits, and decide the claims of innumerable vintages from all quarters of the globe. One great contest lies between the so-called "natural wines," purporting to be, but not always being, the unadulterated juice of the grape, which, though of late pushed with the greatest vigour, and perhaps in time to gain the ascendency, have not yet displaced the older favourites—the class of so-called "fortified" wines, those that are strengthened with added and extraneous alcohol to adapt them, as some say, to the depraved taste of the British consumer, but as others judge, with perhaps more truth, to enable the wines to bear the voyage to this country, and to secure their keeping qualities in our climate. Into that controversy we shall not here enter. There is no doubt that now and for some time to come the ordinary consumer will prefer, as a matter of taste or habit, those wines which have been so long the established beverages of this country—the sherries, ports, madeiras, clarets, champagnes, and their congeners, still holding their own against the vintages of Austria, Greece, Italy, Australia, and other new introductions. These fortified wines, however, differ most essentially among themselves as regards quality and purity. Some are strengthened with brandy, itself a product of the grape, and so far suitable and congruous; others with a highly rectified and pure alcohol prepared from grain, and quite as well fitted for the purpose, while too many of the cheap wines are doctored with a most inferior and dangerous spirit distilled from potatoes, beetroot, or rye, full of fusel oil and other impurities, and, worst of all, this dangerous form of alcohol is even used as the basis of an artificial compound sold as wine, without containing a single atom of the juice of the grape in its composition. For medical purposes it is essential that these impure and noxious beverages should be excluded by the physician, and it is because we think this is fairly provided for by "The Prescription Wines" we are about to notice, that we have taken some trouble to test them thoroughly, and have decided that they deserve the consideration of the profession, not as the result of any individual taste, but after deliberate and combined examination by several of our coadjutors capable of judging of the suitability of these wines for medical use. Our attention was recently directed by one of the most respected members of the profession to the "Prescription Wines" of Messrs. Dunbar and Co. of Weymouth Street, and these gentlemen having afforded us the most unreserved information on the subject, we have submitted their selections to very strict tests. In several cases where we suggested alterations or improvement in the qualities, these have been attended to, and now that their wines are ready for public use we have again examined them, and believe them fully worthy of adoption by the profession. The wines so selected are—an excellent dry *Sherry,* a sound *Port,* a well-flavoured and full-bodied *Madeira,* a good plain *Claret,* and a fair *Champagne,* all well suited, in our opinion, for ordinary and medical use. The wines, we have reason to know, are bought direct from the most eminent shippers and importers, and, quality considered, the prices are very moderate. There is one most important point that Messrs. Dunbar and Co. attend to—the maintenance of uniformity in quality, purity, and strength. In their prospectus they say: "To secure in future an identity, and so enable us to give a warranty of uniform strength and purity under 'The Adulteration Act, 1875," we have, in our contracts, provided that sealed and certified samples of the original selections shall be set aside; that all future supplies shall at least equal them; and that in the event of deviation, on the complaint of any consumer, we may be entitled at once to have it determined and remedied by the principal of the Inland Revenue Laboratory, that department being now the final referee under the Act. Having taken these precautions to secure a continuous supply of a perfectly sound article, we further earnestly desire that if at any time our wines do not give satisfaction or appear equal to our warranty, they should be returned at our cost and risk; it being a principle of our business, and shall always remain so, to take back, without question or cavil, any of our supplies and return the money."

HUNYADI JANOS MINERAL WATER.—It is more than a year since we first received a circular regarding this water, but we threw it aside and gave no farther heed to the matter, until during a recent visit to Vienna we heard a mineral water termed "Ofner water" more frequently prescribed than any other. The preference given to it was due, we were told, to its taste being more pleasant, while its efficacy as a purgative equalled, if it did not surpass, that of its rivals. Our curiosity was excited, and we made farther inquiries, from which we learned that this favourite Ofner water and our old acquaintance Hunyadi Janos were one and the same, the term "Ofner" being derived from Ofen, the name of a part of the town of Pest near which the source is situated. The analysis of Baron Liebig shows that this water contains a very large proportion of sodium and magnesium sulphates, and a trial of the water which we have ourselves made leaves no doubt on our minds of its efficiency as a purgative.

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⁂ Any of the foreign works may be procured on application to Messrs. DULAU, of Soho Square, W.C.;—WILLIAMS and NORGATE, of Henrietta Street, Covent Garden, W.C.; or BAILLIÈRE, of King William Street, Charing Cross.

Department of Public Health.

THE REPORTS OF THE MEDICAL OFFICER OF THE PRIVY COUNCIL AND LOCAL GOVERNMENT BOARD FOR 1874.

THREE recently-published blue-books, Nos. IV., V., and VI., of Mr. John Simon's new series of Reports, give an account of the proceedings of the Medical Department of the Local Government Board during the year 1874. The first of these reports refers to the work of that Department in its home relations that year; the second, to the work in certain of its foreign relations; and the third, to the scientific investigations made under the direction of the Lords of the Council in aid of pathology and medicine. Each of these reports presents much matter for consideration of great interest and moment, but in the present article attention will be given to the two first-named reports only.

Mr. Simon describes the action taken by the Local Government Board through its Medical Department as of two sorts. The first consists in collecting and methodising information wanted for general public use or for the increase of the Board's own efficiency with regard to most sanitary questions on which the Board has to advise or direct. The second consists in practical communications with local authorities with an immediate view to the better prevention of diseases in each of the respective jurisdictions. In addition, the Board takes cognizance of the progress of foreign epidemics, partly with regard to any existing possibilities of their extending to this country or its dependencies, and partly with reference to the quarantine conduct of other countries.

With regard to the first sort of action, Mr. Simon begins by a reference to the inquiry conducted by Mr. Netten Radcliffe in 1874 on the prevention of excrement nuisances in towns and villages. Mr. Radcliffe, it appears, for the purpose of this inquiry, visited forty-nine carefully-selected places in Great Britain with a view to collect all the best contemporary experiences on the subject. His report of this inquiry would have its place rightly in the present issue of Mr. Simon's reports, but it was held advisable as of "so much immediate interest for local authorities" to include it in the previous issue. The report will now be familiarly known to our readers, and there are few probably who will not concur in Mr. Simon's expression of "the grateful sense" which he entertains "of the service which Mr. Radcliffe rendered in devoting himself for many months with indefatigable patience to details which only the hope of real public usefulness could make attractive." Since Dante and his companion traversed the second gulf of Malebolga, two such repulsive journeys have rarely been entered upon as those undertaken by Dr. Buchanan and Mr. Radcliffe in 1869, and by Mr. Radcliffe in 1874, for the study of excrement nuisance preventions. These journeys, however, have proved that it is by the detailed study of filth alone that we can hope successfully to arrest the mischiefs arising from it; and Mr. Simon and his coadjutors have rendered no greater service to sanitary work in England than by showing that the study of filth in its different varieties demands for its successful pursuit a scientific training, and that in the interests of the public health it deserves and requires such study. The main difficulty which besets the question of abating excrement nuisance in England at the present time is, we believe, its offensiveness to educated people, and their consequent unwillingness to consider it seriously. No more important agency in removing this natural indisposition to give serious thought to so repulsive a subject can well be conceived than the action which has been taken with regard to it first by the Privy Council, and next by the Local Government Board through its Medical Department. By dealing with the subject through the help of its most experienced and highly-trained officers, both departments of the Government have shown an appreciation of its magnitude and gravity which must influence very considerably the attitude of the educated classes towards it.

Of the collected and methodised information contained in Mr. Simon's recent reports, the greater part relates to vaccination; and of this part the larger portion consists of a peculiarly important report by Dr. Seaton on the recent (1870-73) epidemic of Small-pox in the United Kingdom, in its relation to Vaccination and the Vaccination laws. Other reports included in Mr. Simon's report are one by Dr. Buchanan on Infectious Diseases in Birmingham and Aston, and on the hospital accommodation of those places; and one by Dr. Ballard on the sanitary condition of Upper Sedgley Urban Sanitary District. The first-named report (so valuable for the information it contains as to the influence of removal of cases of infectious diseases to hospital in the limitation of epidemic outbreaks) was reviewed in the *Practitioner* at the time it was communicated to the local authority at Birmingham. The second-named report is an instructive illustration of local sanitary mismanagement.

Dr. Seaton's report gives an account of the great pandemic extension of small-pox, of which the outbreak in Great Britain formed a part, and compares the results of its prevalence in various continental countries with those which occurred in our own. The report abounds in valuable matter of especial importance at the present time, when so much activity is being displayed by certain reckless associations in attempts to unsettle the confidence of the public in vaccination. The general results of the report are stated by Mr. Simon in these words:—

> "The small-pox which the United Kingdom suffered during those 22 years was part of a world-wide prevalence of the disease. A generalisation, which from the earliest times has been familiar to the medical profession, is, that the same diseases at different periods of time make epidemics of widely differing degrees of malignity; and as regards the present matter, it seems universally testified by skilled observers that no small-pox epidemic in living memory had been (if I may so express it) of equally malignant intention with that which is here in question. Both in tendency to spread and in severity of individual attacks, it tested to the very utmost the value of such defences as had been provided against it by vaccination; killing the unvaccinated in far more than ordinary proportion; killing also in unusual proportion persons (especially those beyond the period of puberty) whose previous vaccination had been imperfect; and even prevailing more or less against many vaccination defences which would have borne the stress of any ordinary epidemic. The small-pox deaths in England during the 22 years of this epidemic were 44,433 in number; and while, to persons who were agitating against the practice of vaccination, it seemed a momentary triumph to boast how "in spite of vaccination," this very sad mortality had befallen us, to others, and

particularly to such as were well-informed in the history of small-pox, the reflection occurred, that *except for vaccination* this epidemic would presumably have caused such frightful and demoralizing mortality as the worst pestilences of past centuries used to bring with them: mortality, in comparison with which even those 44,433 deaths were but few. To demonstrate how true was the latter opinion was of course not possible at the time; for it would require that accumulated statistics of the epidemic (statistics which could not be obtained till two or three years had elapsed) should be investigated with particular regard to the influence which vaccination had exerted; but in a case of this immense interest it seemed due to the general public that the investigation should when possible be made, and the teachings of so great an epidemic be set forth: and such is the purpose of Dr. Seaton's laborious and valuable report which I have the honour of bringing under your consideration. Rendering an account of the late epidemic in each of the three divisions of the United Kingdom, and in various countries and chief towns of continental Europe, and combining with such small-pox statistics as can be given in each case particulars as to the local laws and arrangements for vaccination, Dr. Seaton shows that the experience of this epidemic was in harmony with all previous experiences as to the protective power of vaccination. The belief which governs the vaccination system of this country—(that properly-performed infantine vaccination, duly renewed at puberty, will virtually extinguish small pox as a fatal disease among such populations as have recourse to it)—has long rested on so vast a basis of well-established facts as not to have needed, for scientific completeness, that any further facts should corroborate it; but, for persons not already familiar with the proofs which gave rise to our present practice, Dr. Seaton's instructive report may adequately serve instead of those proofs to justify our practice as it stands. Anyone can gather from the report the convincing illustrations which it contains, how unabatedly terrible is still the force of small-pox in the present age, unless its ravages have been precluded by vaccination; and in adverting to that fact I am glad to be able to connect with it the gratifying reflection that against this one preventible disease England is now employing with systematic efficiency, and with success which is in constant increase, those happily-adequate means of resistance which the genius of an English surgeon first added to the resources of mankind."

The second sort of work done by the Local Government Board through its Medical Department—that of practically communicating with Local Authorities, with an immediate view to the better prevention of disease, is somewhat anomalous in its arrangements. Small-pox, by special legislation, is dealt with apart from other preventible diseases, hence a division of the departmental work, in this category, into that which relates to small-pox and that which relates to other diseases. The principle which underlies the action of the Legislature, whether as to small-pox or as to other diseases, that of prevention, is the same, but the difference in practice is amazingly diverse. Measured by the action of the Local Government Board, this difference is hardly credible. The Boards of Guardians are the

Local Vaccination Authorities of the kingdom. They number, it appears, 647, and have under them at work for the performance of vaccinations more than 3,000 medical practitioners acting as public vaccinators, and for the non-medical part of the business about 1,400 vaccination officers. The work of these vaccination authorities and their officers is carried on under precise instructions from the Local Government Board, and it is conducted under the systematic supervision of the Medical Department. In this way an efficiency is maintained which has no parallel among other nations, and which appears to be secured without any undue friction between the central and local authorities. The whole method of public vaccination is carried out upon a well-conceived and consistent plan, which contemplates and secures the utmost value in efficiency for the money expended upon it from both the local and the central funds.

If we turn now to the action of the Local Government Board through its Medical Department as to the prevention of diseases other than small-pox, we find a very different state of things. The areas of separate sanitary jurisdiction in England and Wales, including the Metropolitan areas, are 1,558, in the greater number of which the sanitary authorities are aided by the State, through the Local Government Board, in the payment of their sanitary officers. The State, indeed, contributes some £50,000 for this purpose. The practical supervision exercised by the Local Government Board over the administration of the Sanitary Acts in 1874, is represented, according to Mr. Simon, by 56 medical inspectors—17 in rural and 39 in urban sanitary districts. Of these inspections, Mr. Simon gives a tabular list, and referring to them, he observes:—"The above communications neither enable me to submit to you any adequate materials for discriminating as to the progress of disease-prevention in the districts of England, nor, I apprehend, can have represented an exertion of the Board's influence in favour of sanitary progress in more than a small fraction of the cases where such progress is urgently wanted."

It would appear, then, that as in the initiation of the Public Health Act, 1872, the Local Government Board excluded its Medical Department, the source especially provided by the Legislature for its information on the subject-matter, from all participation, advisory or other, so, in the administration of those Acts, it limits that participation to an amount little short of a vanishing point. Neither the responsibility which rests upon the Board for a judicious expenditure of the money it gives in aid of sanitary authorities, nor the efficiency of the health administration which is entrusted to it, appears to weigh with the Board. The difference between the efficiency of the administration of the Vaccination Acts and of the Sanitary Acts is the difference of a system founded upon a competent knowledge of the subject-matter, and of a system founded upon an amateur knowledge,—in other words, between trained knowledge and quackery. The method of administering the Vaccination Acts was the work of the Privy Council; the method of administering the Sanitary Acts is the work of the Poor-Law Department of the Local Government Board. The difference is significant. When a theoretical conception brought about the union of Poor-law with Sanitary Administration it frustrated genuine sanitary work for many years, unless Poor-law administrators had proved other than they were—namely, human.

Of progress in sanitary work made in England under the administration of the Local Government Board, Mr. Simon thinks there is strong presumption from, among other considerations, the increase in the amount of money borrowed by local authorities for sanitary works. Tne average annual sum borrowed during the six years, 1858-64, was £330,000. From 1870 to the end of 1872 (twenty-nine months), the annual average exceeded £384,000. Mr. Simon fails to distinguish between progress made from the direct operation of the Sanitary Acts and from the administrative action of the Local Government Board; and he writes as if the Local Government Board were to be credited with the progress. We congratulate Mr. Simon on having so much faith left in him as to the possibility of his Board having even accidentally promoted good sanitary work.

Mr. Simon, however, does not fail to point out how in a very large proportion of our local jurisdiction there is conclusive evidence as to the insufficiency of sanitary endeavour; and he makes the following important observations on the subject:—

"Evidently it is for the Board alone to determine how far it holds itself responsible for taking cognisance of local successes and unsuccesses in regard of the prevention of disease, and for influencing or endeavouring to influence towards better sanitary administration those Local Authorities in whose districts the unsuccess betrays itself. I am of course aware that this question is not one to be judged from an exclusively sanitary stand-point, but is part of the general ordering of our civil government, and that justly to decide regarding it is the function, not of medicine, but of statesmanship. In appealing, however, to the latter, I trust that I may not be speaking with an improper prejudice in favour of that great human interest for which it is the privilege of my office to be permitted to speak, if I assume on its behalf that, among the many administrative purposes for which central government in this country claims to criticise and influence and in certain cases control the action of Local Authorities, none can be of more national importance than those which relate to the prevention of disease,—none, in regard of which it can be more essential that local disobedience to positive law should not be tolerated,—none in regard of which it can be more desirable that, so far as occasion exists, and to the extent of existing knowledge and skill in the subject-matter, the detailed influence and advice of central government should be given to promote throughout the country the real intentions of the Legislature. And in behoof of these last-mentioned considerations (though with all due reserve and diffidence as regards those to which I first adverted) I have thought it essential to put before you, as matter of proportion, how slight, except in regard to small-pox, is the cognisance which the Board hitherto takes, and how far apart are its exertions of skilled influence, in regard of those local defaults of disease-prevention which annually cost many tens of thousands of lives.

"In my last Annual Report, submitting to you some remarks with regard to the relations of the Board to Local Sanitary Authorities, and particularly with regard to such of those relations as attach to the Medical Department, I explained the scope and spirit of such medical inspections as I would recommend the Board to adopt as its means of sanitary superintendence. Presuming as principles, first, that local government in proportion as it fulfils its purpose must be exempt from central interferenee, and secondly, that central interference where admissible must rather be little than much, I submitted, as within those principles, that the Board's intervention should proceed methodically from the basis of local death-accounts: from the evidence, namely, which is afforded in such accounts as to the degrees in which preventible disease has not been prevented. And while recognising of course that in certain of these cases the object of the Board's intervention could be attained by correspondence, I particularly dwelt on the very great advantage which, at least for many years to come, would in my opinion be gained to the sanitary progress of the country by central inspection systematically organised with reference, so far as needful, to such local defaults as a class:—"By skilled inspections under the Public Health Act, 1858, on such a 'scale as practically to represent a central audit of local death-accounts, and an 'exertion of central influence or authority in favour of more active or better-'directed local efforts in places where human life had not been sufficiently cared 'for, the Local Government Board would be in reality, though only in the limited 'sense which the law prescribes, a Board of Sanitary Superintendence for 'England." Such examination of local sanitary proceedings as is hitherto undertaken by the Board is on the basis above suggested; but with regard to the question of its present proportion to local shortcomings, I may state that the quantity of medical inspecting-force available to the Department for this branch of service is but about equal to the employment of three Inspectors; that the sanitary jurisdictions of England, are, as above enumerated, 1,558; and that the mortality, other than by small-pox, to be accounted for (of course in very different proportions) by the 1,558 local Authorities, as due to causes more or less under control of law, and as principally attaching to those two great fields of disease-causation which I have specially mentioned, amounts, in the common estimate of informed persons, to at least 120,000 deaths per annum.

With regard to about 900 of the 1,558 jurisdictions, the Board is entitled to receive copies of the reports of local Officers of Health, appointed under the Order of the Board, and paid in half from funds provided by Parliament; and there are considerable parts of the country with regard to which it may be ascertained from such reports that the local Authorities are already receiving excellent advice from highly-qualified officers whom they have appointed, and that in many cases they are duly acting upon such advice. But as regards the relations of the Central Board to the facts of sanitary unsuccess which become known to it, the following three considerations have to be remembered; first, that in certain cases local Authorities, though provided with admirable Officers of Health, may be entirely disregarding their advice; secondly, that in very many of the cases in which the Board is entitled to receive copies of the reports of the local officers, those which come to hand are of little or no assistance to the Board in the sense which is here in question, but rather give reason to believe that the local officer might be very materially assisted by the visit of a medical Inspector of the Board; and thirdly, that, with regard to about 658 of the 1,558 jurisdictions, the Board (as not paying part of the local salaries) is not entitled to receive copies of the local reports.

It importantly concerns the second of these considerations to notice that, at the time when the Legislature suddenly made it an obligation on each of the local Sanitary Authorities of England to appoint one or more Officers of Health, the members of the Medical Profession who had given such study to sanitary science and administration as would fairly qualify them to be Officers of Health in any distinctive sense of the term were comparatively but very few; that therefore the new appointments had generally to be made without reference to proofs of distinctive qualification; and that in a large proportion of cases, the local Officer of Health, appointed as such with the approval of the Board, is in fact (as the Royal Commission recommended he should be) the Poor Law Medical Officer serving *quasi ex officio* in the second capacity. When regard is is had to these facts, it will be evident that the advance of the new officers to a state of efficiency in which their reports may uniformly be regarded as adequate to the circumstances of the respective districts, must of necessity be a gradual process, extending over years, and requiring the best assistance of suitable organisation. And the fact (of which the Medical Profession has great reason to be proud) that the new offices are already in so many cases filled with credit, and in a notable number of cases with really eminent ability and success, must not obscure the fact, that, before such standards can be general in the country, very much work will have to be done, and very many difficulties surmounted."

Of the Foreign work of the Local Government Board, carried out by the Medical Department in 1874, it is represented in these reports by a brief account from Mr. Simon's pen of recent appearances of plague, an abstract by Dr. Seaton of the proceedings of the International Sanitary Conference held at

Vienna in July–August of that year (Dr. Seaton, together with Dr. Dickson of Constantinople, having attended that Conference on behalf of her Majesty's Government), and by a Report of Mr. Netten Radcliffe on the Diffusion of Cholera, and its prevalence in Europe during the ten years, 1865-74. Mr. Radcliffe's report forms the chief bulk of Mr. Simon's new series, No. 5; but for the present we shall limit ourselves to certain observations of Mr. Simon as to cholera; simply repeating his statement with regard to the most considerable and important part of Mr. Radcliffe's report—namely, the account of the great diffusion of cholera from 1865 to 1868—that it has been "studied with a completeness perhaps never before attempted in regard to so large a passage of epidemiology."

Mr. Simon, after commenting on the various important questions arising out of Mr. Radcliffe's report, proceeds then to consider certain other questions which do not come within the scope of that report, but which he holds desirable to consider in connection with it. We quote, from their importance, the observations of Mr. Simon just referred to:—

> "Cholera, as a geographical subject-matter, divides itself for our practical purposes into three sections: (1) cholera, as existing in India; (2) cholera, as diffusing to other countries; and (3) cholera, as we here, in England or generally in Europe, see it from time to time epidemic among us.
>
> Persons regarding exclusively the second of those sections, and seeing it as now displayed by Mr. Radcliffe, might at first take an almost fatalistic view of the immense mischief which cholera inflicts on mankind. For they see the disease diffusing itself as an attribute of such vast social movements as it is evidently impossible to control or minutely scrutinise: spreading itself, namely, in all directions in those constantly-moving streams of religious pilgrimage and commercial enterprise which constitute very large proportions of national life; and they see that a first, but unattainable, condition of dealing completely with a contagion thus distributed would be that they should be able to immobilize at their discretion the great tides of human life which convey it. They would no doubt see that the marine exportation of cholera from India may be considerably impeded by an enlightened and vigorous supervision of shipping, such as Her Majesty's Indian Government have designed under the Acts (constantly in course of amendment) which relate to native passenger-ships; but they would fear that, even if only in such movements of land-population as comparatively escape control, means must continue by which cholera existing in India will have ample facilities for extending to Europe; and they would recognise how the progress of circumstances is constantly making it less and less possible that, at times when cholera is present in any territory of Europe, the other territories should succeed in really protecting themselves against it by restrictions on international movement.

Undoubtedly, therefore, it is in directions which are outside the view of Mr. Radcliffe's present paper, that we must mainly look for such hopes as can be entertained of dealing effectively with this great pestilence. We have to hope, on the one hand, that India may be made less productive of the infection: we have to hope, on the other, that other countries may be made less susceptible of it. And in each of these directions, there is room for very hopeful work.

Of the conditions which in Europe make local susceptibility to the infection of cholera, I have often had occasion to speak. That cholera, when imported into a locality, will under certain circumstances spread from the sick as from a centre, is among the certainties of medicine; but we know with at least equal certainty that its means of thus spreading are strictly limited, and the limiting conditions which are best known to us in regard of it are those which bring it into intimate analogy with our own enteric fever, and justify us in classing it as a Filth-Disease. On the preventibility of cholera in this point of view, I have said so much on other occasions that I need not now insist on it, and would only refer to the general report which I had the honour of laying before you a year ago on the subject of Filth-Diseases and their Prevention.[1] It cannot I think reasonably be doubted but that, as conditions of filth, and especially as filthy conditions of water-supply, are the main facilitating conditions for the dissemination of cholera in Europe, so they must be immensely potent influences in favouring the advance of cholera from station to station in successive epidemic outbreaks in the countries which lie between India and Europe; and it would seem certain that, along the whole succession of lands which transmit the streams of westward traffic from India, common hygienic vigilance in respect of those conditions may be of very great effect in impeding the diffusion of cholera.

Regarding cholera in India, apparently two sets of conditions have to be distinguished: (*a*) the conditions of the disease's endemic prevalence, in larger or smaller quantity, in a limited portion of the Indian continent, and (*b*) the conditions of its occasional spread over areas indefinitely larger.

That under the latter head must be counted at least in part the same property of contagiousness, and the same relation of this property to circumstances of filth, as we knew to be the essential conditions for the extension of the disease in Europe, is an opinion which European pathologists find it impossible not to entertain:[2] and so far as that European opinion holds good, the amount of cholera-diffusion in India is likely to be controlled by the same preventive measures as are certainly effectual for the like purpose in Europe. Those parts of the world therefore, which have had the habit of receiving cholera-infection from India, and especially those which have received it from secondary centres of infection, may expect to incur less and less of this danger in proportion as common sanitary government shall more and more bring within control the common Filth-Diseases of that vast country. And in this point of view it is most hopeful to know that for many past years, and even more rapidly than Europe has been able to appreciate her own interest in the sanitary progress of India, the British Government has been organising in that part of the empire such provisions for sanitary government as promise soon to compare not unfavourably with the present institutions of Europe itself.

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[1] Report, New Series, No. II.

[2] If this opinion is less generally entertained in India, or in parts of India, than it is in Europe, probably the difference of opinion is in great part only an expression of the fact that it must be more and more difficult to prove the contagiousness of the disease in proportion as studies on this point are made in places where the disease may be autochthonic.

"The distinctive Indian problem, however, is that of the endemicity of cholera in part of the Indian area. It is from within British Territory that this enemy of the human race sallies forth from time to time to do world-wide execution; and the first threat of each coming foray is that the disease is produced in greater quantity in the area to which it primarily belongs. There, in Lower Bengal—specially in the Sunderbunds and other alluvial lands from Juggernauth to beyond the mouth of the Bhurmpooter, cholera is said to be as normal to the district as ague to the marsh-lands of Europe, and as definitely related to the easons as if it were a fruit of the earth. To learn the conditions of that endemicity and its variations is a problem of the highest science; and to control them in the interests of mankind will be among the noblest administrative tasks which the responsibilities of empire have ever imposed. It would be foreign to my province to dwell upon this part of the subject: but I may permit myself to advert to the fact that Her Majesty's Government, now for some years past, has been promoting the study of cholera in India with the best lights of contemporary European knowledge: and no one will doubt but that here, as in other departments of medical research, truly scientific study must eventuate in practical good.

Briefly then,—if the constantly-developing and constantly-accelerating commerce between India and the rest of the world is not to carry with it a constantly-increasing terror of pestilence, the safeguards, I apprehend, will consist, not in contrivances of the nature of quarantine to maintain from time to time more or less seclusion of nation from nation, but rather in such progressive sanitary improvements on both sides as will reduce to a minimum on the one side the conditions which originate the infection, and on the other side the conditions which extend it. That cholera, as endemic and epidemic in India, will even in the near future, be rendered by the exertions of the British Government far less terrible to Europe than it has been for the last half-century, is a conclusion which think cannot fail to be entertained by those European epidemiologists who are conversant with the annual volumes, printed by order of the Secretary of State for India in Council, with regard to the measures adopted for improvements in India."

PROFESSOR PARKES, M.D., F.R.S.

THE death of Professor Parkes is a national calamity. It occurred on the 15th ult. The history of his life and labours must be an afterwork. The immediate consequences of this loss are so great that they occupy the mind to the exclusion of other considerations. It will be hard to fill his place at the General Medical Council of Education, and harder still to fill it at Netley. Long and thoroughly versed in all that goes to make the accomplished medical man and in the necessities of a medical training, he possessed an influence in the Medical Council as great as it was desirable. Knowing medicine as one of the lucrative sciences (*Brodwissenschaften*), he sought to promote its practice as an art consistently with its dignity as a profession, not by sacrificing the latter but by raising the standard of the former. As the ablest teacher of hygiene in this country, the civil population suffers from his loss alike with the Army and the Navy. Through his great work on "Practical Hygiene" he exercised as large and beneficial an effect upon the study and practice of hygiene in the civil medical schools of the kingdom as that which he exercised upon military and naval medical men by his teachings at the Army Medical School at Netley. England has gained very much from those teachings; India has also gained much, and is gaining more. The time has not yet come when the gain to India from Professor Parkes's teachings at Netley can be rightly estimated. For the moment his loss with regard to sanitary work in our great dependency seems irreparable.

Much as the public and the profession suffer from Professor Parkes's death, his personal friends suffer more. Few men begot such warm attachments. He has died as greatly lamented as honoured, leaving an enduring name.

THE PRACTITIONER.

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Original Communications.

ON THE PROCESS OF FEVER.

BY J. BURDON SANDERSON, M.D., LL.D., F.R.S.

(*Continued from* p. 280.)

PART II.—PRODUCTION AND DISCHARGE OF HEAT IN FEVER.

Section I.—Experimental Investigations.

THE constancy of the temperature of the body depends upon the existence of heat equilibrium, *i.e.*, of that condition of the organism in which the processes by which heat is produced, and those by which it is liberated, balance each other. It is certain that increased activity of the former is not in itself a sufficient cause for high temperature, for we know that the thermogenetic functions undergo frequent variations, some of which are accidental, while others take place regularly, without the occurrence of any corresponding changes in the temperature of the body. Thus after a meal, a variety of heat-producing processes come into operation which were before in abeyance; again, during and after muscular exercise the quantity of material disintegrated is much greater than in repose; but in neither instance is there any permanent elevation of the bodily temperature, because in both the increased production is balanced by the increased liberation of heat at the surface. In like manner it can be shown that an unnaturally high temperature may be maintained although the production of heat is diminished. Thus, it is well known that after certain modes of death, the temperature of the body does not sink for some time after the circulation has ceased, notwithstanding that the heat-producing processes which are dependent on the supply of oxygen are no longer in operation.

Heat stands on the same line with carbonic acid, urea, and water, as a product of chemical work done in the living body. To determine whether or not its production is increased or diminished, we have to proceed by continuous measurement just as in the other cases, with this difference, that the measurement of heat is a much more complicated and difficult problem than that of any of the chemical products of life. There are two methods by which it may be attempted. The first consists in estimating the thermogenesis from what is known as to the quantity and "heat value" of the material daily and hourly consumed in the body, under the conditions to be investigated; the second, in directly measuring the quantity of heat daily or hourly discharged from the body, this quantity being, if the temperature is constant, identical with the quantity produced. In employing the first plan, that of estimation, we depend entirely on certain experiments made about eight years ago at the Royal Institution, by Prof. Frankland (the accuracy of which has been generally admitted), by which the "heat value" of the "immediate principles" of food (albumin, fat, and some carbonic hydrates), *i.e.*, the quantity of heat yielded by each in complete or partial oxidation, was estimated.[1]

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[1] Of the values obtained, the most important and the most frequently use are those relating to albumin and its product urea, and to fat. A gramme of albumin, according to Frankland, yields 4·998 kilogramme-units of heat in complete combustion, *i.e.* 4·998 times as much heat as is required to raise a kilogramme of water one degree of temperature. A gramme of urea yields 2·206 kilogramme-units; a gramme of fat 9·069 kilogramme-units. In the disintegration of albumin in the living body, it does not yield the ultimate products (water, ammonia, and carbonic acid), but nearly the whole of its nitrogen passes out in the form of urea. Consequently in estimating the quantity of heat generated by it in the organism (its "physiological heat value") we deduct from its total heat value, the heat value of the weight of urea which is derived from it. Now, each gramme of

Leyden found, as has been already seen, that his fever patients exhaled during the remission, *i.e.*, when free from fever, 83·8 litres (at 0° C. and 760 m.m.) of air in 15 minutes, which contained 3·3 per cent., *i.e.* 2·79 litres of carbonic acid. A litre of carbonic acid weighs 1·9712 gramme. Consequently the discharge of carbonic acid per 15 minutes was 5·5 grammes, or 22 grammes per hour. This gives 528 grammes as the discharge per day. In fever the same patients exhaled 134·6 litres in 15 minutes, containing 3·066 per cent. of carbonic acid, or, 4·127 litres. This gives 32·5 grammes per hour, or 780 grammes in 24 hours, supposing the rate of discharge to be constant. Senator, from determinations made in cases strictly comparable with those of Leyden, estimated the daily discharge of urea in patients on fever diet, but free from fever, as 17·5 grammes. We may therefore take 17·5 grammes of urea (representing 52·5 grammes of albumin), and 528 grammes of carbonic acid, as an approximation as near as can be attained to the true estimate of the discharge of a healthy male person on fever diet.

On these data we may proceed as follows:—The physiological heat value of 52·5 grammes of albumin is 229·0 k.-units. The 52·5 grammes contain 27·82 grammes (53 per cent.) of carbon, of which 3·5 take the form of urea in order to leave the organism (for urea contains one-fifth of its weight of carbon, and 17·5 grammes are discharged). Deducting the remainder of carbon, (*i.e.*, the quantity not so discharged) from 144 grammes (the quantity of carbon contained in 528 grammes of carbonic acid), we have 119·68 grammes as the quantity of carbon to be accounted for as derived from other sources. Now in inanition or on fever diet there is but one non-nitrogenous source of carbonic acid which we have to consider, namely, the fat of the tissues, consequently it is from fat that the 119·7 grammes of carbon must be derived. Taking the per-centage of carbon in

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albumin yields one-third of a gramme of urea, that being the quantity which would be produced by it if all its nitrogen were, in passing out of the body, to enter into the constitution of urea, for whereas albumin contains 15·5 per cent. of nitrogen, urea contains 46·66 per cent., and $\frac{15\cdot5}{46\cdot66} = \frac{1}{3}$. Hence of the total heat value of every gramme of albumin consumed physiologically, as much as belongs to one-third of a gramme of urea (*i.e.*, $\frac{2\cdot206}{3} = 0\cdot735$ k.-units) is lost to the organism. Deducting this from 4·998, we have 4·263 as the "physiological heat value" of albumin.

fat as 76·5, we have 156·4 grammes as the weight of fat, which must have been consumed in order to produce the quantity of carbonic acid actually discharged. According to Frankland's estimate 156·4 grammes of fat yield in disintegration 1419 k.-units of heat. Adding this to the quantities derived from the disintegration of albumin we have 1648 k.-units as the total quantity of heat produced by patients on fever diet but in the apyretic state.

By substituting for the numbers given above relating to the discharges in health those relating to fever, and repeating the process, we arrive at a comparable result as to the febrile production of heat. In fever, according to Senator's estimate, the urea discharge is increased to about 40 grammes daily, *i.e.*, it is about two and a third times as great as it would be on the same diet in health. Leyden's estimate of the carbonic acid discharge has already been given as 780 grammes daily. The physiological heat value 120 grammes of albumin (the quantity which corresponds to 40 grammes of urea) is 511·56 heat units. The 120 grammes contain 63·6 grammes of carbon, of which 8 grammes leave the organism in the form of urea. The remainder of carbon (55·6 grammes) having been deducted from 212·7 grammes, the total carbon-discharge by respiration (*i.e.*, the quantity of carbon corresponding to 780 grammes of carbonic acid) we have 157·1 grammes as the weight of carbon to be accounted for by the consumption of fat in the body. The weight of fat required for this purpose is 205·3 grammes, which would yield 1862·4 k.-units. Adding this, as before, to the quantity of heat derived from the disintegration of albumin, we have 2373·9 as the total heat production of fever.

Ranke found in his experiments on himself that on an adequate mixed diet, *i.e.*, on a diet sufficient, and not more than sufficient, to maintain nutritive equilibrium, he discharged in twenty-four hours a quantity of nitrogen corresponding to 32·3 grammes of urea, and that his respiratory discharge of carbonic acid was 791 grammes. Proceeding as before, we have 413·5 k.-units as the quantity of heat yielded by the disintegration of 97 grammes of albumin, which in this case was of course derived from food. Of the carbon contained in this 97 grammes, 45 grammes would have to be discharged in carbonic acid. Deducting these from the total discharge of carbon, viz., 215·7 grammes, we have 170·7 grammes of carbon to be accounted for as derived from the non-nitrogenous constituents of food. The diet consisted of 250 grammes of meat (containing a very small proportion of fat), 400 grammes of bread, 70 grammes of farinaceous food, 70 grammes of egg-albumin, and 100 grammes of butter and lard. From previous determinations it was estimated that the fat of the meat contained about 2·8 grammes of carbon, the butter and lard about 67·9 grammes, the farinaceous food about 26 grammes. This leaves 74 grammes to be accounted for as having been derived from the bread, for 2·8 + 67·9 + 26 + 74 = 170·7. 170·7 grammes therefore represents the balance of carbon in the expired carbonic acid not already accounted for as derived from the disintegration of albumin. (The actual quantity of carbon contained in the carbonic hydrates of the bread was 80 grammes, so that we have an excess of 6 grammes unaccounted for.) According to Frankland's table the fat would yield 33·19 k.-units, the butter 852·7 k.-units, the bread and other farinaceous food (supposing them to contain 156·5 grammes of starch of which the heat value is 5·232), 819 k.-units. Adding these to the 413·5 k.-units derived from the disintegration of albumin, we have 33·19 + 852·7 + 819 + 413·5 = 2118·39 k.-units as the heat-production of a healthy adult on a mixed adequate diet. On similar data derived from other experiments on himself, Ranke estimated his own mean heat-production on adequate diet at 2,200 k.-units.

Thus we have for the three conditions we have been considering, namely, inadequate or fever diet without fever, inadequate diet with fever, and adequate diet in health, the following results:—

Inanition	1648·0 k.-units.
Fever	2373·9 k.-units.
Health	2118·4 k.-units.

The general result to which the preceding calculation leads us is a very important one, namely, that, although as compared with the heat-production of an individual on fever diet, the heat-production of a fevered person is excessive, it is not by any means greater than the heat-production of health, for the highest difference indicated by the numbers stated is insignificant, as will be more fully explained hereafter.

The results of Professor Leyden's observations as to the discharge of carbonic acid in various acute febrile diseases to which our attention was directed in the first part of this paper, afford ground for concluding that, although as compared with the heat-production of an individual on fever diet, the heat-production of a fevered person is excessive, it is not by any means greater than the heat-production of health, for the highest difference indicated by the numbers stated is, as we shall see immediately, insignificant.

In estimating the value of this result, there are several considerations to which it is requisite to call attention. In the first place, it is to be noticed that the data employed as representing respectively the discharges of nitrogen and of carbon in fever, are the highest that could be taken; thus, those relating to urea were founded on observations of fevers of short duration, and referred to periods during which the characters of the febrile state showed themselves in their fullest intensity. It is still more important to remember that the estimate of the febrile discharge of carbonic acid in 24 hours is founded on determinations relating to the rate of discharge during the day only. In comparing the results with those relating to the same patients when free from fever, this error was got rid of, for both sets of observations were made in exactly the same way. Consequently the numbers given above, representing the relation between heat-production on fever diet without fever, and on the same diet in the febrile state, may be regarded as accurate; but if we compare either of these numbers with that representing the heat-production of health with adequate diet, a correction is required.

Taken absolutely, both of them are unquestionably too high, for it is well known that the rate of carbonic acid discharge is considerably higher in the day than in the night, so that any estimate of the total discharge from measurements made only during the day is certain to be excessive. Pettenkofer and Voit found that in health the mean discharge during the whole 24 hours falls short of the mean rate during the day by 14 per cent. If we make a deduction of 14 per cent. from the estimated febrile discharge of carbonic acid which was taken as the basis of our estimate given above, of the heat-production in fever, we have to take off 109 grammes from our total of 780 grammes. Now the heat-discharge corresponding to each gramme of carbonic acid derived from the consumption of fat is 3·23 k.-units; consequently, if in fever the difference between day and night is as great as in health, we must take off 352 (= 3·23 × 109) k.-units from our estimate. Thus corrected the numbers stand thus:—

Heat-production in fever on fever diet -	2021 k.-units.
Heat-production in health on adequate diet	2118 k.-units.

It is further to be borne in mind, that the state of things which is understood by the term "adequate diet" is not that of ordinary life. By adequate diet is meant a diet which is just sufficient to maintain nutritive equilibrium, *i.e.*, to balance expenditure by income. Under ordinary circumstances we consume a great deal more food than is required for this purpose. In Professor Ranke's experiment, the diet of a young man of 24 consisted, as we have seen, of half-a-pound of meat and a pound of bread, besides small quantities of butter and eggs, &c., an amount of aliment which, although it was proved experimentally to be "adequate," would in ordinary language be described as insufficient, and is certainly very inconsiderable as compared with the usual requirements of persons of the same age and sex. From the results of his experiment on more abundant dietaries Ranke inferred that the activity of the thermogenetic processes of his body could be increased to as much as 2,700 k.-units per diem, an amount far exceeding the highest estimate that could be made of the possible production of heat in fever.

Up to the present time no one has attempted to determine the quantity of heat produced in the human body by *direct* calorimetrical measurement. The only approach to such measurement is to be found in the clinical observations of Prof. Leyden as to the relative quantity of heat given off by the skin in fevers and in health, of which an account will be given in the next section; but in the dog, as has been already stated, the total heat-production has been made the subject of an elaborate investigation, extending over many years, by Dr.

Senator. As these measurements are the only ones which have as yet been made of this function in the higher animals, it will be necessary to give the reader a sufficiently complete account of them to enable him to understand their bearing.

In Dr. Senator's, as in all other methods of physiological calorimetry, the quantity of heat given off by an animal in a given time is ascertained by measuring the loss of heat at its surface; and this measurement is effected by placing it in a ventilated chamber of which the wall is hollow and contains water.

The calorimeter consists essentially of two copper boxes, an internal and an external, resembling each other in form. The internal serves for the reception of the animal, and is provided with a cover which closes air-tight; the external contains the water, to which heat is to be communicated from the body of the animal. The inner case is suspended in the water in such a way that it is in contact with water on all sides, so that the animal occupies an air cavity which is completely surrounded by water. This cavity communicates with the external air by two ventilating channels, one for the entrance, the other for the discharge of air. The temperature of the inner box is measured by two thermometers, its contents being thoroughly agitated before the measurement. The whole apparatus is contained in a wooden case, between which and the external surface of the water-box there is a packing of non-conducting material, in order to diminish as much as possible the loss of heat. As a preliminary step, this loss was determined in a number of experiments in which the difference between the temperature of the water contained in the chamber and that of the environment varied as much as possible. From these a series of corrections for "temperature difference" were obtained. In all the experiments the chamber contained the same quantity of water, 37 litres. From the weight and specific heat of the metal contained in the boxes their calorimetrical value was estimated as equivalent to that of 2·5 litres of water, so that the total water-value of the apparatus was 39·5 litres. The term "coefficient of cooling" is used by Senator to indicate the number of kilogramme-heat-units lost by the apparatus in one hour for any given number of degrees of "temperature difference." This was learnt by multiplying the correction for difference by 39·5. The quantity of heat in k.-units lost by the air passing through the apparatus was of course obtained by multiplying its estimated weight by the specific heat of air (0·237) and the increase of temperature (*i.e.*, the difference of temperature between the air entering and that passing out of the apparatus). The ventilation was effected by a Bunsen's water-air-pump and measured by a gas-meter; from this measurement the weight was estimated, the proper corrections for pressure and temperature having been made. The methods by which the carbonic acid and water were determined have been already described. From the hourly discharge of water the quantity of heat lost by evaporation was estimated.[1]

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[1] In calorimetrical, as in all other quantitative measurements of the products of life, it is necessary that for each result two experiments should be made, of which one is the exact counterpart of the other, with the exception that in the former the condition to be investigated is present, in the latter absent. All Dr. Senator's observations were made on this principle, that is, each measurement of the febrile heat-production of an animal was compared, not with an estimated normal, representing the mean results of a number of similar experiments on other animals, but from an experiment made previously on the same animal under exactly similar conditions, fever excepted. Considering, however, how important it is that a normal standard for future use in pathological inquiries should be obtained, he has recently made a series of determinations on a sufficient number of healthy animals, with this object in view. I subjoin a summary of one of these observations, which may serve as an example of the method employed in the whole research.

A dog, weighing 4,270 grm, which had been kept for four weeks on a diet of 250 grammes of selected horseflesh, with five grammes of lard, and 100 cubic centimetres of water, was introduced into the calorimetrical chamber at 9.34 a.m. The temperature of the animal when introduced was 39°·0 C., and that of the water of the calorimeter 27°·35. It remained in the chamber for four hours, during which the temperatures of the calorimetrical water, of the air on entering and leaving, and of the surrounding air, were observed every 10 minutes. The results of each hour were reckoned separately as follows: the calorimetrical value of the calorimeter, (*i.e.*, the quantity of heat required to augment its temperature by one degree) being 3·94 k.-units, and the increase of temperature during the hour being 0°·02 C., the heat communicated to it by the animal was 0·79 k.-units. Secondly, 144 litres of air having passed through it, of which the temperature was raised by 4°·48, the heat communicated to it (= the product of the weight of 144 litres at the temperature at which it entered, the specific heat of air, viz., 0·237, and the increment of temperature,) was computed as 0·18 k.-units. Thirdly, the air on entering the calorimeter being dry, and having taken up from the animal 0·75 gramme moisture, this quantity, multiplied by 0·582, gave 0·44 k.-units. Lastly, the quantity of heat given off by the apparatus to external media as previously determined empirically, for the difference of temperature

With these preliminary statements we may proceed to examine Dr. Senator's experimental results as to the production of heat in the fevered organism of the dog. With this view, comparative observations were made on the same animals in respect of which those relating to the discharge of carbonic acid and urea have already been given (p. 262); the measurement of heat-production and of carbonic acid discharge being carried out simultaneously, that is, during periods of observation of which each lasted an hour. As was before explained, the normal heat-production of each animal was first ascertained by measuring it on two successive days of privation of food. The observations were then repeated, after the animal had been brought back again to nutritive equilibrium, under conditions which differed only in one respect from those of the first, namely, in the previous induction of the febrile state by the injection of fresh pus.

The following is a tabular summary of the results stated in the same form as in Table I.

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between the temperature of the calorimetrical water and that of the external air was 11·22. Hence the total quantity of heat given off by the animal to its surroundings during the hour (=0·79 + 0·18 + 0·44 + 11·22) was 12·63 k.-units. Similarly estimated, the discharges of heat in k.-units for the second, third, and fourth hours were severally 12·26, 11·54, and 11·92. From 26 similar measurements of six healthy animals, the following results were obtained in k.-units as representing the heat-production of the dog per kilogramme of body weight, viz., 2·34, 2·71, 2·24, 2·88, 2·18, 2·85, giving a mean of 2·53 k.-units as the quantity of heat produced by a dog when fasting during summer.

In experiments relating to the influence of food on heat-production it was found that the same dog which in the fasting state yielded 23·28 k.-units of heat per hour, and 5·2 to 5·5 grammes of carbonic acid, produced six hours after feeding 35·43 k.-units and 9·5 grammes of carbonic acid, its temperature remaining the same throughout. In this, as in other observations, the food given was the daily ration of flesh and fat which was sufficient to maintain nutritive equilibrium.

TABLE II.

TABLE SHOWING THE QUANTITY OF HEAT produced per HOUR by FEVERED DOGS as compared with the QUANTITY by the same ANIMALS when free from FEVER but under the SAME CONDITIONS of NOURISHMENT.

		Bodily Temperature (in rectum). Means.		Heat Production.	
Observation 1.					
Weight of Animal, 11 lbs. 10 oz.					
1st Day	Normal	39·0		13·32	
	Fever	39·3		12·46	
2nd Day	Normal	39·0		11·50	
	Fever	40·3		11·58	
Observation 2.					
Weight, 13 lbs. 4 oz.					
1st Day	Normal	39·1		15·67	
	Fever	39·4		15·29	
2nd Day	Normal	39·1		17·32	
	Fever	40·3		15·57	
Observation 3.					
Weight, 16 lbs.					
1st Day	Normal	39·0		12·64	
	Fever	39·6		9·91	
2nd Day	Normal	38·8		11·87	
	Fever	40·7		14·52	
3rd Day	Fever	40·7		11·87	
Observation 4.					
Weight, 10 lbs. 10 oz.					
1st Day	Normal	39·3		8·67	
	Fever	39·5		9·59	
2nd Day	Normal	39·3		10·24	
	Fever	40·7		11·86	
3rd Day	Fever	39·6		9·43	
Observation 5.					
Weight, 9 lbs. 2 oz.					
1st Day	Normal	38·9		12·31	
	Fever	39·7–40·3		11·97	
2nd Day	Normal	38·9		12·67	
	Fever	41·0		15·22	
Observation 6.					
Weight, 24 lbs.					
1st Day	Normal	39·0		24·18	
	Fever	39·2		25·46	
2nd Day	Normal	39·0		24·48	
	Fever	40·0		23·59	
Observation 7.					
Weight, 12 lbs. 9 oz.		Morning.	Afternoon.	Morning.	Afternoon.
1st Day	Normal	38·8	38·8	15·94	16·20
	Fever	38·7	40·7	15·34	17·44
2nd Day	Normal	38·8	38·6	16·47	17·06
	Fever	40·0	40·0	15·48	19·50
3rd Day	Fever	40·4	39·9	17·41	15·57

A glance at the above results is sufficient to show that they afford no ground for concluding that in the induced fever of the dog there is on the whole any increase of heat production. One or two instances no doubt, as for example the observations relating to the second day of fever in the first series, and to the third day in the third and seventh series, might be referred to in favour of increased production; for in these cases the discharge of heat was increased, at the same time that the temperature of the animal rose. But in others, as in those relating to the second day in series 2, 6, and 7, although the temperature in each instance remained unaltered, the calorimetrical result was decidedly less than normal.

In series 7, the observations were more complete than in any of the others. The period during which the heat production and the discharge of carbonic acid was measured was extended to four hours, of which three were consecutive (10 A.M. to 1 P.M.), and the fourth separated by a four-hours' interval (5 to 6 P.M.). The "insensible loss" was also determined for each day of observation. The insensible loss was increased more than 50 per cent., it being 85 and 75 grammes respectively on the first and second normal days, and 142 and 151 grammes during the corresponding days of fever. All of this increased loss of weight must have been due to increased discharge of water, for the determinations of carbonic acid for the same days show that the rate of discharge of this product was rather diminished than increased. Notwithstanding this fact, the weight of aqueous vapour taken up during each hour of observation by the air transmitted through the chamber, was not greater in the fever series than in the others. As, however, in all the observations the total quantity of aqueous vapour discharged by this channel (two or three grammes) is shown by the detailed results of the experiments to have been small as compared with the very considerable "insensible loss" per hour, the discrepancy may perhaps be attributed to the insufficiency of the ventilation employed (about 150 litres per hour).

Dr. Senator concludes his discussion of his calorimetrical results as follows:—

In the first stage of fever the discharge of heat is rather diminished than increased, so that at this period it is *probable* that there is an abnormal retention. As the febrile process progresses, and when it is at its height, fluctuations exhibit themselves in the quantity of heat liberated, which resemble those which are observed in the discharge of carbonic acid and of aqueous vapour ; but it cannot as yet be determined whether the amount of heat given off during the whole course of the febrile process is greater or less than the quantity of heat given off during the same period of apyrexia.

In judging of the significance of the fact last stated, it must be borne in mind that the normal with which the febrile thermogenesis is here compared, is that of inanition. In the dog, when on adequate diet, the production of heat is at least 50 per cent. more active. If, therefore, we were to take the animal in the ordinary condition of nourishment as our standard of comparison, we should find the heat production in fever very considerably diminished.

Section II.—Clinical Investigations of the Discharge of Heat from the Skin.

It has been seen in the last section that in the fevered animal, in respect of which the conditions are such as to admit of exactitude of measurement, such measurement affords no support to the hypothesis that increased thermogenesis is a characteristic of the febrile state. In man no attempt has yet been made to measure the heat production of the whole body. It has, however, been concluded from the results of observations as to the emission of heat from the cutaneous surface in those fevers which come under clinical observation, that the production of heat in the whole body is augmented. Here, again, as with regard to the discharge of carbonic acid, the pathologist depends almost entirely on the observations of Professor Leyden.

The series of researches to which we have now to refer were made in 1868.[1] They were directed exclusively to the relation between pyrexia, loss of weight, and discharge of heat from the surface in fever, the subjects of observation being for the most part persons affected with relapsing fever, typhoid fever, or pneumonia.

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[1] Leyden. Untersuchungen über das Fieber. Deutsches Archiv. vol. v. pp. 273-371.

For the investigation of the surface loss of heat, a water calorimeter was employed, which was constructed on the same principle as that already described as applicable for the investigation of the discharge of heat in animals, with the exception that there was no provision for the continuous passage through it of a current of air. The apparatus is of such form and dimensions as to contain one limb instead of the whole body.

The copper chamber in which the limb is contained during the observation is two feet long and one foot wide. It is surrounded by a cylinder of zinc of corresponding form, but from three to four inches wide. The outer wall of the water chamber is protected from abstraction of heat by a thick padding of non-conducting material inclosed in a wooden case. The equal distribution of heat in the water contained in the chamber is secured by two agitators, turned by a winch, which revolve round the outer surface of the cylinder, so as to maintain a continuous current in the direction of rotation. The open end of the chamber is lined by an annular air cushion of india-rubber, which, when the limb is introduced, occupies the space between its surface and that of the copper, so as to close the chamber air tight. The part of the body observed is the leg, the lower extremity being introduced into the chamber to such a distance that the india-rubber cushion gently presses round the knee immediately above the joint.

Each observation lasted two hours. Before beginning it the water in the calorimeter was brought to the temperature of the room, the variations of which were of course noted. The greatest pains were taken that the limb, as placed in the chamber, should be under otherwise normal conditions, and particularly that it should be carefully supported on a kind of wooden tray contained in the chamber, so as to avoid contact with the metal, and that it should be clothed with blanket of the same thickness as the leg of the opposite side. When these precautions were attended to, it was found not only that its introduction into the calorimeter did not at all interfere with the comfort of the patient, but that the conditions and temperature of the limb were at the end of the two hours the same as those of its fellow.

The mode of estimating the results of the observations was as follows:—According to Funke,[1] the surface of a grown person is 2,254 square inches, that of the leg alone 149 square inches, the two numbers being in the ratio of 15 : 1. The calorimetrical value of the chamber is 55 k.-units. The mean rise of temperature produced in an hour by the introduction of the limb of a healthy person was 0°·12 C. Hence 6·6 k.-units (55 × 0·12) must have been given off to it by the limb in one hour. Assuming that the quantity given off was in the same proportion to its surface, we have 99 k.-units as the hourly heat discharge, *i.e.* production of the body. This corresponds pretty closely with the estimate already given of the normal heat discharge in health, viz., 2,200 to 2,600 k.-units; for 24 × 99 = 2,376. This is probably high; for if we add to it the 400 k.-units which according to Helmholtz's estimate are given off by the lungs, we have a total of more than 2,700 k.-units for the total heat loss.

The following is a tabular summary of the most complete of the series of observations reported in Professor Leyden's paper. It relates to two cases of relapsing fever and two of pneumonia. The table scarcely requires explanation. In the first case the observations were made during the accession, during the first remission, and the following relapse; in the second case the observations were begun during the first remission, and continued during the second relapse and the second remission. In the table each line gives the result of an observation; the patient's temperature and pulse are stated in columns 4 and 5, while column 6 shows the rise of temperature produced in the calorimetrical water in the course of one hour. As has already been stated, it was found that in a healthy person the mean rise of temperature produced in the course of one hour was 0°·12 C. when the ward was at the ordinary temperature, viz., 17°·3 C. (63° 1 F.)

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[1] Quoted by Leyden. I am not able to ascertain in what paper of Funke's these facts are recorded.

TABLE SHOWING the RESULTS of OBSERVATIONS relating to the DISCHARGE of HEAT from the SURFACE of the SKIN in FEVER.

1. No.	2. Date.	3. Temperature of ward.	4. Temperature of patient.	5. Pulse.	6. Increment of temperature.	REMARKS.
						CASE I.—RELAPSING FEVER.
1	Oct. 19	18·8	40·0	120	0·19	M. æt. ? Weight 96 lbs. The periods of observations 2 and 3 followed at an interval of half an hour, during which a rigor occurred. State of skin not noted. Observations 5 and 6 were made during the relapse at 12h. 15m. and 1h. 45m. of the same day. Before observation 5 the patient had had a rigor, and the skin was hot and dry. At 1h. 15m. sweating came on, which continued during the period of observation. Observation 7 was made at 5h. 35m. the same day; observation 8 at mid-day, when convalescence was established.
2	,, 29	18·1	40·5	124	0·14	
3	,, 29	18·1	?	?	0·18	
4	,, 30	18·5	40·3	124	0·15	
5	,, 31	19 7	41·4	120	0·13	
6	,, 31	19·8	39·5	not stated	0·20	
7	,, 31	19·9	36·1	80	0·06	
8	Nov. 8	17·5	37·3	80	0·105	
						CASE II.—RELAPSING FEVER.
1	Nov. 14	20·5 mean	39·8	108	0·16	M. æt. 18, weight 101 lbs. The numbers bracketed relate to a period of observation of 5 hours, during which the bodily temperature was gradually sinking. The patient was sweating the whole time, most profusely dur-the middle hour, when, as seen in column 6, the surface loss of heat was greatest. Observations 2 to 6 were made on different days, during the non-febrile interval. Each observation lasted two hours, of which the mean result is given in each case. The patient was perfectly free from fever. Observations 7 and 8 were made at 11 A.M., and 6 P.M. of the first day of the relapse. The skin was dry and hot, moaning constant, excessive thirst. In the afternoon the temperature rose to 41·0.
			38·7	88	0·19	
			—	—	0·24	
			37·2	64	0·16	
			37·2	—	0·15	
2	,, 15	20·5	36·8	64	0·075	
3	,, 16	20 3	36·5	68	0·10	
4	,, 17	20·5	36·5	64	0·06	
5	,, 18	20·0	36·5	68	0·10	
6	,, 19	20·0	36·5	68	0·10	
7	,, 20	19·6	40·6	106	0·145	
8	,, 21	19·4	40·4	108	0·145	
9	,, 22	19·4	39·0 to 40·7	110 104	0·142	
10	,, 23	—	40·5	not stated	0·10	
11						
12	,, 23	19·5	38·9	104	0·20	
	,, 23	—	37·4	88	0·18	

1. No.	2. Date.	3. Temperature of ward.	4. Temperature of patient.	5. Pulse.	6. Increment of temperature.	Remarks.
						Observation 9 was continued for four hours, viz. from noon to 4 P.M. During the whole time the skin was hot and dry. Observation 10 lasted two hours, commencing at 11 A.M. The skin was hot and dry. Observation 11 lasted also two hours, commencing at 5h. 45m. P.M. In the course of it the patient began to sweat, and continued to do so all the evening. At 8h. 15m. observation 12 was taken.
						CASE III.—PNEUMONIA.
1	Jan. 8	18·8	40·0	100	0·192	
2	,, 8	18·8	39·2 to 37·2	92 to 76	0·26	M. æt. 19. Weight 101 lbs. Observation 1 was made at mid-day, the skin being moist.
3	,, 11	18·6	37·1	not stated	0·105	Observation 2 at 6h. 30m. P.M., the perspiration being general over the whole body. Observation 3 relates to convalescence.
						CASE IV.—PNEUMONIA.
1	Jan. 11	19·5	39·7 to 40·5	100 —	0·14	M. æt. 30. Weight 130 lbs.
2	,, 12	19·9	40·3	100	0·175	
3	,, 13	19·5	39·3 to 38·4	100 —	0 225	
4	,, 14	19·5	40·2	108	0·23	
5	,, 20	17 0	Normal.	Normal.	0·11	

Before proceeding to examine the results contained in this table, it will be advantageous to state the signification attached to them by the author and accepted by other authorities on the pathology of fever. Dr. Leyden expresses his conclusions as follows :—“ The discharge of heat is increased in fever whether the temperature is constant, falls, or rises. Consequently it is certain that the production of heat is increased. In high fever, the quantity of heat given off is from half as much again as the normal to twice as much. The most rapid discharge of heat takes place in the critical stage when the temperature is rapidly sinking. It may then be twice or even three times as great as the normal. Defervescence is associated with marked sweating and evaporation at the surface; fervescence by the absence of these phenomena, even when the coverings are air-tight."[1]

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[1] In dealing with the question we have now before us, Senator, in the paper already so often quoted, finds in Leyden's researches the most important basis for the construction of his doctrine of pyrexia. In beginning his chapter on this subject he says:—"By Leyden's observations these two facts come out prominently. 1. That with the exception of the initial stage, the discharge of heat is considerably increased, although by no means constantly. 2. That the activity of the discharge is not proportional to the bodily temperature, for it may be less when the temperature is high than when it is lower; it may be normal when the temperature is above the normal, always attaining its maximum in the stage of deferverscence with critical sweating," &c. He then, after referring to the confirmatory results of Liebermeister and Hattwich, and giving an explanation of the apparent contradictions between Leyden's general conclusion that in fever the production of heat is nearly doubled, and the contrary results obtained by himself in his calorimetrical measurements of fevered dogs, proceeds to observe that notwithstanding that the observations of Leyden extended at the utmost over a few hours, and related only to the discharge of a part of the body, it appears to him to be permissible to found conclusions upon them, so long as it is understood that they are accepted subject to drawbacks, which may turn out to be more important than they at first sight seem. "We may therefore," he says, "estimate the average increase of heat loss in febrile pyrexia as compared with the normal as 70-75 per cent. As, moreover, at this time the constant temperature of the body is higher, it must have at its disposal in pyrexia an even larger quantity of heat than is required in the non-febrile state to maintain itself at fever temperature; a quantity which not only exceeds that produced by a healthy person in inanition or on fever diet, but even that generated in ordinary conditions of nourishment. In making this estimate it must, however, be expressly stated that it applies only to the condition of fervescence, and that it must not be taken as representing the total loss of heat in the process, from the beginning to the end; for during the defervescence the loss is much greater, and it is much less during the accession." Senator's general conclusion on the subject is stated as follows:—"The discharge of heat is in the outset of fever during the rigor, not increased but diminished; during the height of the fever it is, on the whole, increased in the ratio (so far as it relates to the day hours) of 70-75 per cent., and considerably more during the critical defervescence. During the height the greater part of the loss occurs by conduction and radiation, during the crisis by evaporation."

Results corroborative of those of Leyden are furnished by the observations of Liebermeister as to the quantity of heat given off by a fever patient in the tepid bath, as compared with a healthy person. Regarded as calorimetrical experiments, these measurements are subject to objections which cannot now be considered.

These passages serve to show that Senator and Leyden, the two greatest autho-

The question we have before us is not whether the fact that in febrile pyrexia the temperature is as a rule increased both in man and animals by two or three degrees is due to this or that cause, but simply whether in fever along with the increased temperature there is also increased production.

That *cæteris paribus* there is in fever an increased activity of *certain processes* which produce heat can be proved without calorimetrical measurement. Supposing the state of the heat-discharging surfaces to remain the same, the fever patient must necessarily produce more heat to keep his body up to the higher mark as regards temperature than the healthy person. In other words, supposing the quantity and quality of clothing, and the temperature and moisture of the air to remain the same, and the discharge of watery liquid from the sweat glands to be also the same, the fevered person would give off a somewhat larger quantity of heat from the surface than a healthy person; for the difference between the temperature of his body and that of the environment being greater, the loss by radiation and convection would be proportionally greater. The difference, however, would certainly be small, greater somewhat in summer than in winter, and greater in bed than in clothing.

It is also certain that the process of disintegration of tissue, of which the evidence has already been presented to us, is in its nature thermogenetic. In the preceding section I have estimated that increase, and have shown that there is unquestionably a very considerable difference between the heat production of a febrile and of a non-febrile person on the same febrile diet. I have also pointed out that this holds true only as long as the comparison is made under similar conditions of nutrition, for if we compare the heat production of a fevered patient with that of a person in health on ordinary diet, we find that whatever

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rities on the pathology of fever, are agreed in accepting the fact as certain, that production of heat is largely increased, and that both of them found their conclusions simply and exclusively on Leyden's cases, for although both of them refer to Liebermeister's observations in confirmation, each indicates some of the reasons for regarding the results of that observer as inapplicable to the question. If, therefore, it can be shown that Leyden's facts admit of a different interpretation, the whole foundation of the very generally accepted belief that the heat production is greater in fever than in health under normal circumstances of nutrition, falls to the ground.

difference presents itself is quite within the limits of normal variation.

The fact of the greater consumption of material in fever may therefore be dismissed entirely from our minds in entering on the consideration of observations similar to those of Professor Leyden; for in those observations the comparison between fever and health is made, not in regard of the discharge of heat from the whole body (pulmonary and cutaneous), but in regard of the quantity of heat liberated by a given area of skin in fever and in health. Whatever may be the result of a comparison between these two conditions as regards the activity of the process by which heat is given off by the skin, there is no reason for supposing that a well-fed person would, if the skin were in a natural state, emit more heat from a square inch of surface than an ill-fed, or that the quantity of heat given off by the skin is dependent on any other cause than on changes in its own temperature or moisture, or in that of the atmosphere with which it is in contact.

The careful study of Professor Leyden's results has led me to an interpretation which differs materially from that which he has embodied in his main conclusion. He admits throughout the great importance of visible perspiration, *i.e.*, of the secretion of watery liquid by the sweat glands as a condition favouring the discharge of heat from the skin. He points out that in all those of his experiments in which the heating of the calorimetrical water was most rapid, the result could be connected with rapid cooling of the accessible parts of the body, and with profuse sweating. But he finds that there were other cases in which, notwithstanding the dryness of the skin, the fevered body parted with its heat to the calorimeter with a rapidity which could not possibly be accounted for as the mere result of the greater heat of the surface. In looking through the cases, I am unable to find a single instance in which, the state of the skin being noted, it was found that in the absence of perspiration the loss from the surface was considerably in excess. This being so, I am compelled to associate increased discharge from the surface, not with pyrexia, but with sweating, for while on the one hand I find instances in which the patient was in high fever, with only an average of heat loss, I find in the same patient on another day a very active discharge of heat from the surface, but no fever.

In so far as it can be shown that the increased rate at which the fever patient's heat was communicated from the limb to the calorimeter in which it was inclosed is dependent on sweating, the result is of little value or significance as an index of increased production of heat in the living tissues. Under the conditions of the experiment, *i.e.*, when a limb is inclosed in an air-tight chamber, the air which occupies the space between the cutaneous surface and that of the chamber soon becomes saturated with moisture. As soon as this state of things is established there is no further loss of heat by the conversion of sweat into vapour; the effect of sweating, therefore, resolves itself into the mere abstraction from the limb of a certain quantity of watery liquid, of which the whole of the heat goes to the good of the calorimeter. So far as the body of the patient is concerned, the process is attended with the loss of a certain quantity of water, and manifests itself in a corresponding loss of weight; but so far as relates to the chemical processes by which heat is produced, it fails to afford any information. If for every gramme of water sweated out at the surface it were the law of the animal economy that an equal quantity of cold water should be ingested, then it might be said with truth that for every gramme discharged a quantity of heat must be generated in the body sufficient to warm a gramme of water from the ordinary temperature to that of the blood. So far from this being the case, the loss of water is, as a rule, supplied in the diet of fever by liquid, of which the temperature is as high as, or higher than, that which it has to acquire in order to be discharged, in which case it is obvious that the water, as it actually leaves the body cooler than it entered it, must (in so far as it has any appreciable action on the temperature of the body), tend rather to favour the accumulation of heat than to promote its discharge.

Professor Leyden's observations are of great value, as affording a more complete understanding than we before possessed of the function of the sweat glands in defervescence. In this respect some of the observations (as *e.g.* the second case in the table) are particularly striking. In that instance, as in many others, it was seen that the temperature of the patient's body sank very rapidly as that of the calorimeter rose, the transference of heat from one to the other being effected either by the trickling down of drops of hot moisture from the limb on to the floor of the chamber, or by a process of distillation through the air, in which for every quantity of water condensed on the metal surface a similar quantity is evaporated from the skin. In those parts of the surface which are not inclosed in a chamber it is clear that the sweating must be more effectual than in the chamber itself; for here the air which is in contact with the skin is not saturated, so that, in addition to the loss of heat by the discharge of warm sweat, there is an additional loss due to the conversion of the whole or part of it into aqueous vapour. In either case the activity of the process is dependent entirely on the secreting function of the sweat glands, *i.e.*, on the quantity of watery liquid which they throw out on the surface, and has no direct connexion with any heat producing or other nutritive processes having their seats in the tissues. Whether the watery discharge is converted into vapour or soaks into the patient's blankets, it is discharged without any arrangement existing or being required for its continual replacement; so that, if I may be permitted to use the expression, the loss of heat by sweating is payment out of capital; it may go on for a very long period without any appreciable effect excepting loss of weight. For this very reason, of all functional fluctuations, those of sweating are of least value as outward indications of inward changes.

(To be continued.)

ON FEBRIFUGES.

BY C. BINZ, M.D. LOND.,

Professor at the University of Bonn.

(*Continued from* p. 290.)

THERE is one point not directly connected with the consideration of alcohol as an antipyretic, but bearing upon the treatment of fever patients in general, which is of such importance that I cannot pass it over without notice. This is, *the value of alcohol as a food in fever cases.* I do not know that I shall be able to bring forward any evidence upon this point new to the English reader, but as the opinions I hold are based upon my own experimental researches, an expression of the same may contribute toward supporting a theory which many English medical men agree with me in advocating, and which as many others tenaciously oppose.

We must regard as a food any substance which, when taken into the system, can serve (1) towards building up the tissues, or (2) towards supplying the warmth and vital force necessary for the proper performance of the various functions of the body.

As the list of positive examples is so extensive, these points can better be illustrated by reference to certain negative ones. Thein, for instance, is not a food, as it passes through the system without undergoing decomposition ; nor is ether a food, since it cannot be taken in quantities sufficiently large to contribute by its oxidation any appreciable weight, warmth, or vital energy[1] to the body. Gum arabic is not a food, because it is not absorbed in the intestinal canal.

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[1] Ether, it is true, acts as a cardiac stimulant, but as such it contributes no *new* force to the heart ; all it does being to excite the heart so as to make it put forth what force it already has more energetically. In other words, instead of contributing fresh power, it drains away more rapidly that which is left.

Alcohol fails, perhaps, to fulfil the first office of a food according to the foregoing definition, since it is incapable, as far as we know, of supplying material to build up the tissues, but when given in small doses, oft repeated, especially in the case of a sick person, it may be said to surpass all other substances as a species of easily burning fuel, from whose combustion the heat required to generate vital force may be derived. Indirectly, it also answers the first of the aforesaid purposes, for though it may furnish actually no new building material, it spares the reserve supply of fat in the body, which would otherwise have to be burned to give the necessary warmth. In sickness, where but little or no fatty material is supplied by the food, this is of course conservative, but in health no need is felt for a specially combustible form of fuel, and so we see how many persons in good health, under the combined influences of good living, alcohol and bodily inactivity, grow very fat, the alcohol supplying a good deal of the heat required by the system, and some unburned fat continuing to be stored away year after year.

I have convinced myself by a series of experiments that alcohol is completely destroyed in the animal organism.[1] If pure it leaves no taint upon the breath, and where this is present it is attributable to some of the ethers or fusel oil. Thus, in the experiments referred to, I found only traces. I used three different methods: the chromic acid and the iodoform test, and Geissler's Vaporimeter. Their correctness was always controlled by known dilutions in water containing most minute fractions of alcohol. The urine may, under very favourable circumstances, contain as much as three per cent. of all the alcohol consumed. This is the highest figure I have been able to get. Aldehyde and acetic acid, the derivatives of alcohol, were absent. The facts observed all seem to lead to the conclusion that alcohol in the body, just as in the flame of the spirit-lamp, is oxidised to carbonic acid and water.

Such being the case, it is evident that every molecule of alcohol burned within the system must yield, not only warmth, but that power to accomplish work with which the development of caloric is always accompanied. Let us see how much.

From direct experiments made by Favre and Silbermann, and by Frankland in England, with a view to ascertain the relative heating power possessed by various combustible substances, gases, &c., it was found that alcohol gave the figure 7; pure coal gave 8, and hydrogen 34·5; which is to be understood as meaning, that one cubic centimeter of alcohol generates heat enough during its combustion to raise the temperature of seven litres of water 1° Cent.; that one cubic centimeter of coal sufficed to raise eight litres 1° Cent., and so on. The unit of the scale thus arbitrarily set up is represented by the amount of heat required to raise the temperature of *one* litre of water 1° Cent., and to this unit the name *Calorie* has been applied. A healthy adult produces about 2,300 such *calories* a day. Now, in the process of consuming 100 cubic centimeters of absolute alcohol, equal to one litre of good hock, we set free within our organism 700 *calories*, or nearly one-third of the whole amount of warmth which is produced by the system under mixed diet within twenty-four hours.

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[1] Cf. *Centralbl. f. d. Med. W.* 1875, p. 371. Berlin.

With this result given by alcohol, let us now compare the heat-producing power of certain other ingesta as determined by Frankland: for instance, cod-liver oil, a substance which may be taken here as a type of the fatty elements of food in general, being one of the most digestible. Its calorific power is represented by the figure 9·1. Four table-spoonfuls taken four times a day—about 50 cubic centimeters—develop 455 *calories*, supposing the whole quantity to be digested, and this, it will be seen, is only about $\frac{4}{7}$ths of what 100 grammes of alcohol supply, or in other words, not more than would be furnished by 65 grammes of alcohol.

The advantage possessed by the latter over the oil is obvious. Besides being infinitely more pleasant to take, the alcohol, if largely diluted with water, in the form of good wine, is readily digested, even in the weakest stomach; and, as has been shown, it is easily and completely disintegrated in its passage through the system. On the other hand, all fatty substances require for their emulsion, absorption, and assimilation, a very considerable amount of work, which of course is only that much more drain upon the powers of a patient.

Thus we can understand how it is that a continual supply of wine enables the patient to resist for a certain length of time the ravages of a severe disease, where no other form of nourishment is accepted by his stomach. Every practising physician who recognises the value of wine in therapeutics will be able to call to mind cases of typhoid fever, where for several days nothing was taken but wine and water, the patient retaining his strength pretty well all the while. Bricheteau, a French surgeon, relates the case of a boy affected with diphtheria, upon whom the operation of tracheotomy was performed, and who for a month afterwards would take nothing but sweet wine, of which he consumed one-and-a-half bottles daily, besides two ounces of rum, and who during the whole time did extremely well. In this case the sugar of the wine of course contributed something as a food to the whole effect, but being present in the sweetest wine only in small proportion, it could have accomplished only very little.

In such cases it would be unreasonable to suppose that the vital powers of the patient are kept up solely by the stimulating properties of the alcohol, for the nervous system and heart require nourishment as well as any of our other organs, and they cannot be kept going so long by stimulants alone. On the contrary, by so urging them on to activity day after day, without supplying any food to compensate for the wear and tear resulting from such augmented action, it is certain that we would exhaust their forces more quickly than by any other plan of proceeding. Alcohol certainly acts as a stimulant in conditions of extreme weakness, but given for days together when no other combustible material is being supplied to the system, it burns in the tissues, and by means of the heat thus generated, furnishes the body with warmth and the strength necessary to carry on its vegetative functions.

I would especially call attention to the success with which dilute alcohol in frequent doses, in the form of Kumyss,[1] and in a similar manner of other light alcoholic beverages, has been applied for several years past in the treatment of the earlier stages of pulmonary tuberculosis; first in Görbersdorf (Silesia) and subsequently in Davos (Switzerland). If a form of nourishment that fattens,—this and nothing further can be claimed for cod-liver oil,—is a dietetic desideratum of primary importance in combating the commencement of tuberculosis, then the free use of light wines belongs to the treatment, for experience at the two places above mentioned shows that, as a rule, patients fatten upon the plan of treatment there pursued, without suffering from any febrile effervescence or injurious excitement attributable to the abundant use of wine.

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[1] A sparkling wine of about 2 per cent. of alcohol, prepared from milk sugar; the solids it contains are so trifling in quantity as to be insignificant.

And even in the more advanced stages of consumption, a relatively favourable influence from the alcohol treatment can be expected, as was strikingly shown by experiments made on my request in the Bonn military hospital two years ago.[1]

To my surprise, a meritorious author has lately expressed quite a contrary opinion.[2]

"Through every stage, except the first, the special action of alcohol is to reduce the animal temperature. In fact, cold and alcohol act in the same manner on the vital processes . . . . the amount of carbonic acid is decreased. From all these facts the inference is that alcohol is not burned after the manner of a food which supports animal combustion, but that it is decomposed into secondary products, by oxidation, at the expense of the oxygen which ought to be applied to the natural heating of the body."

It seems to me that the inference so drawn, however plausible in theory, is overborne by the following facts elicited from experiments. First, that after *small* doses of alcohol, neither the temperature of the blood nor the quantity of urea in process of being thrown off is found to be measurably diminished; secondly, that even such quantities as lowered the temperature of the blood at first, cease to produce this effect after having been repeated a few times; thirdly, that when there is no impediment to the respiration, the organism obtains much more oxygen than it can consume; and fourthly, that no "secondary" product of alcohol has been found as yet in the organism.

A litre or a half-litre of good hock, taken all in a short time, will produce in a decided manner the results already described as belonging to the action of powerful doses of alcohol; but if given to an adult—even to a "teetotaler"—in broken doses distributed equally along through the whole twenty-four hours, it will not affect the temperature of the body during that time to any measurable extent. In the latter instance it will simply burn, as would any other innocent and digestible hydro-carbon, giving off its proper quantum of heat to the tissues, and resolving itself into carbonic acid and water. A similar proceeding, if repeated the next day, will simply give the same results.

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[1] Virchow's *Archiv.* lx. 471.

[2] Dr. B. Richardson, report in the *Lancet*, 1875, p. 241. A former paper of the same author (1866), mentioned in this report, was previously unknown to me.

Where we have to deal with a patient whose stomach will accept nothing else, and whose absorbent glands will assimilate no other form of nourishment, the nervous system and the heart derive from the alcohol of that bottle of hock or claret a good part of the energy which they exhibit in the continued performance of their functions, for as this alcohol burns, the heat evolved by its combustion *must be* converted into working force.

Alcohol is not the only medicinal agent which produces entirely different effects according to the manner of its administration. In support of this assertion I need only refer to calomel, and to the enormous difference of action it exerts according as it is given in small doses oft repeated, or in single large ones.

One of the most difficult points for the practitioner is to procure a form of alcoholic beverage that is quite free from fusel oil. If, for instance, we take two similar animals and intoxicate one of them with an alcohol which has been previously purified by treatment with wood charcoal just recently made glowing—(such charcoal takes up all the fusel oil)—while the other is made drunk with alcohol to which a few drops of amyl-alcohol have been added, a decided difference of effect will be observed. The latter animal lies completely stupefied, while the former still runs about; the one recovers from its condition of narcosis but slowly and with difficulty,—the other rapidly. If the doses administered have been large, a paralysis of the respiratory centre may readily supervene in the case of the animal to which the impure alcohol was given, while the other, with a like quantity of pure ethyl-alcohol, continues in the enjoyment of life.

There can be no question that the human organism is affected by the fusel oils in quite the same manner. This is clearly observable in districts where potato-spirit is drunk. Such potato-spirit contains a relatively large proportion of fusel oil, and in the districts referred to drunkenness commonly assumes the most desperate character. One sees the same thing in a less degree in connection with the consumption of wines which have been "*doctored*" with alcohol obtained from potatoes. These wines very quickly produce a heavy intoxication, the disagreeable after-effects of which are remarkably protracted, whereas pure wines, such as only the grape furnishes, even in intoxicating quantities leave behind, as a rule, neither headache nor any undue fulness of the cerebral vessels.

Now, if we introduce into the system of a sick man any alcoholic mixture in which fusel oil is contained, the evil effects produced by the latter upon the brain, in this case already weakened by disease, will manifest themselves yet more strongly than they would upon a healthy one; and just here lies the great practical difficulty, the "hidden reef," so to speak, for the therapeutic use of alcohol. After all the different calamities that have befallen the vine upon the Continent, the quantity of impure alcoholic drinks at present in circulation is necessarily very large, thus making it particularly difficult nowadays to find a pure article. How then is the physician to meet this difficulty?

There are two ways open. First, he may employ in his practice only the pure spirit of wine, from which, largely diluted with water, he causes to be prepared a palatable drink, containing in addition sugar and some innocent aromatic substance. To test, however, whether the spirit of wine itself is quite free from fusel oil, it is only necessary to rub a few drops of it between the palms of the hands, thus causing rapid evaporation, after which, by means of the sense of smell, one can decide the question. Pure alcohol evaporates very quickly, leaving behind no smell whatever, while that which is impure leaves upon the hand a distinct odour of fusel oil, which consists principally of amyl-alcohol.

A second plan is as follows. Let the physician make exclusive use of some *one* form of alcoholic preparation which he has found from repeated experience to be reliable; this the patients are to procure always from the same source, best of course from some large and respectable establishment. Then it is further very desirable that specimens of this special preparation should be examined from time to time in order to determine that its percentage of alcohol remains the same. This can be done in a few minutes by means of the Vaporimeter[1] of Geissler, even by those who are inexperienced in chemical manipulations. In the following table I give the percentage of absolute alcohol contained in some of the different well-known spirituous drinks.

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[1] To be had of Dr. Geissler, 14, Burgstrasse, Bonn. Price 1*l.* 10*s.*

Volume per cent. of absolute alcohol contained in Bavarian beer is	4—5
In the different kinds of hock and claret	8—11
In the sweet Spanish and Italian wines	13—17
In brandy, gin, whisky, &c.	30—50

To produce an antipyretic effect an adult will require a dose of not less than 50 cubic centimeters, or about two fluid ounces of absolute alcohol.

By reference to this standard, the dose suitable for each individual case can be easily reckoned out, according to the age and strength of the patient.

Finally, to sum up my views, as far as the same concern the practice of medicine, I may say (1), that alcohol is very frequently a stimulant of transitory power; (2), that in relatively large doses it can serve as a vigorous antipyretic; (3), alcohol, given in small and oft-repeated doses, is a food particularly adapted to cases where the stomach can take in no other "combustible material" to supply warmth and working energy to the organism.

While I thus share in the views which the late Dr. Anstie so ably upheld in England, I do not hesitate, on the other hand, to declare, with respect to the requirements of the healthy organism, that I consider the use of alcohol in health as entirely superfluous.

Where the digestion is healthy, and where a sufficient transfer of nutritive material takes place from the food to the blood, the human body is capable of accomplishing all the functions for which it is designed, and that without the use of spirituous drinks. But the case assumes a different aspect as soon as these two conditions fail. A physician may therefore recommend total abstinence to healthy persons in every instance, but he throws away one of the most valuable of medicines if he carries this principle of *teetotalism* into the sick-room.

(*To be continued.*)

USE OF BLISTERS IN CHRONIC ULCERS.

BY S. D. TURNEY,
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CHRONIC ulcers of the leg are frequently so intractable to treatment that any method which will diminish the number of these troublesome diseases will undoubtedly be acceptable to the profession. After a short but very satisfactory experience, I think I am justified in claiming an unusual curative effect in the application of the Esmarch anæmic bandage.

The first case in which it was used by me was a typical indolent ulcer over the internal malleolus of a woman passed eighty-five years old. This ulcer had resisted all treatment, constitutional and local, by myself and other physicians for over fifteen years. It was almost circular in shape, about two inches in diameter, with indurated edges, and a base showing no healthy granulations. The skin around it was thickened and reddened with an eczematous eruption.

The elastic bandage was firmly applied once a day, from the foot to the knee, and was allowed to remain as long as it could be borne, about ten to fifteen minutes. This was all the treatment—the woman, active for her years, pursuing her ordinary avocations. In a very short time improvement was manifest; her cuticle shot out from the margin of the ulcer, and, very strangely, an island of cuticle sprang up from the centre of its base, and the sore went steadily on to cure in less than four weeks. This is now more than two years ago, and the cicatrix is still firm and has even resisted a severe attack of eczema.

I have since applied this bandage in six cases; five have resulted in what appears to be permanent cure, the other,—

a very large ulcer, involving the whole anterior surface of the tibia—cicatrized, but subsequently a portion of the cicatrix gave way. It was again progressing favourably to cure, when the patient disappeared.

Continuous pressure in the treatment of these sores is familiar to all surgeons. But here is something more than continuous pressure: with each application a fresh change of oxygenated blood takes the place of a fluid unfit for nutrition; the distended blood vessels, completely relieved of their load of vitiated blood, recover their lost tonicity, and the strong pressure most effectually overcomes the passive congestion and œdematous infiltration. It is in this alternation of anæmia and consequent hyperæmia that the cure resides.

ON OFFICIAL DOSAGE, WITH SOME REMARKS ON HOMŒOPATHIC TINCTURES.[1]

BY ROBERT FARQUHARSON, M.D.

Lecturer on Materia Medica at St. Mary's Hospital Medical School, &c.

It is now more than two years since our late lamented colleague, Dr. Anstie, referred, in the pages of the *Practitioner*, to the very uncertain and misleading directions of our official dosage. The text on which his remarks were founded was a case which made a good deal of sensation at the time, and provoked much varied controversy as to the best mode of obviating the disasters which might at any time arise from a similar combination of circumstances.

A physician in Ramsgate, being in attendance on a severe attack of delirium tremens, prescribed half an ounce of tincture of digitalis to be repeated if necessary in four hours, but the chemist, observing that the quantity was eight times larger than the maximum dose allowed by the British Pharmacopœia, refused to make up the medicine as ordered.

The patient died, and, in the inquiry which followed, the medical man expressed his belief that digitalis used according to his instructions might have arrested the disease and saved the sufferer's life. And although the chemist had so far a good line of defence, by appealing to the standard book of official reference in his particular department, he was severely censured by the coroner on the reasonable ground that it was his clear duty to have communicated with the prescriber, and ascertained whether the apparently excessive dose was actually ordered with deliberate intent. Now, although public opinion at that time turned strongly against the man who, by his excess of caution, contributed to so unfortunate a result; and although it was very properly suggested that an unusual dose of any poisonous substance should always be explained by some well-understood mark or symbol, it is impossible not to feel that in this, as in most cases, there is something to be said on both sides. We must remember that great responsibilities devolve on those who are empowered by law to dispense medicines, that the issues of life and death may depend on the slightest slip or mistake, that they may be at any time held liable for serious consequences. On the other hand, we are all of us liable to error, and in our younger days many of us have, no doubt, inadvertently put on paper some hurried combination of drugs in dangerous quantity, which, if made up as written, might have caused an inconvenient or even fatal result. It is clear, therefore, that the deliberate reading over and consideration of our prescriptions by skilful and highly practised dispensers, may be of essential service in correcting mistakes which carelessness or ignorance not unfrequently bring about. And although the British Pharmacopœia was not originally intended in any way to regulate dosage, although information on this score was only added to its second edition "in compliance with a generally expressed wish," and although these directions are not authoritatively enforced by the Council, and "the practitioner may rely on his own judgment and responsibility in graduating the doses of any therapeutic agents which he may wish to administer to his patients," the very fact of the doses being appended at all gives them an official sanction which it is clear was not originally intended. But these directions, such as they are, constitute all which the druggist has to rely upon for information and support under doubtful and exceptional circumstances. Supposing he is called upon to give over into inexperienced hands a bottle of medicine, which his *vade mecum* tells him is far too strong, and supposing at the same time that he is unable to communicate with the prescribing medical man, he is undoubtedly placed in a position of some perplexity. Grant also, however, that the doctor whose prescription is either rejected *in toto,* or mutilated in some way, and the patient whose interests may be gravely imperilled by such a line of action, are, possibly, sufferers both in reputation and in health, and the evidence begins to point pretty plainly to the necessity for some alteration in our present system. I do not suppose I can be singular in my experience in having had prescriptions referred back to me for supposed correction, because I have ordered doses which are well known to be necessary for the proper development of the action of a special drug. Thus, on one occasion when I ordered succus conii in ℥i. doses for acute chorea in a girl of ten, the dispensary prescriber considered it his duty only to give a drachm, and on my next visit pointed out with some concern what he naturally believed to be my mistake. Here no harm was done, for the patient afterwards getting her proper dose was rapidly cured, and the dispenser hailed with satisfaction this addition to his knowledge; but the recollections of my hearers will doubtless bring before them many instances of confusion and delay caused by the fact of our having no properly constituted official guide for prescribing.

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[1] Read before the Medical Society of London.

I will only mention one more instance out of others which I have met with in practice, and that was when, on taking the hospital duty of a friend, I prescribed 20 minims of belladonna for a child of three years old. I shall never forget the consternation with which the pharmaceutical official presumed that I had dashed off at least 10 minims too many in the bustle and hurry of out-patient work, and his surprise when I told him that it was my common custom to prescribe much larger doses with benefit.

Now, although we have seen that the Medical Council did not primarily intend the British Pharmacopœia to be a rigid standard of dosage, and although they also expressly state that the quantities are "intended to represent average doses in ordinary cases for adults," we have also seen that the public have stamped a thoroughly official character on the book, and it has come to this, that the maximum and minimum quantities there enjoined, are now all but universally held to represent the limits within which drugs may be safely or even legally prescribed. Let us, therefore, see in how far the teaching thus laid down is in accordance with modern therapeutics.

To begin then with conium, to which reference has already been made, we will find that the authorized dose of the succus is represented as varying from ♏x. to ʒi. Now the largest of these quantities is probably without any influence on the human frame, and it is hardly necessary for me to remind you that the experiments of John Harley have shown that we must give from one to four or six ounces before we can expect to derive real benefit from the drug, eight ounces even having been reached without ill effect.

Belladonna, again, is allowed in extract only up to half a grain, which is too small, and to ♏xx. of the tincture, which is far within the mark, as I have given from a drachm to ʒij ss. to patients suffering from incontinence of urine with marked advantage.

There is also a curious timidity shown in the directions about quinine, 10 grains of which is held to be the maximum dose. Now, I suppose no fact in medicine is better established than the necessity for administering 20 or 30 grains, or even more, in bad cases of intermittent fever, and the valuable anti-pyretic properties of the drug only come into play when the dose reached is three or four times in excess of that allowed by the Pharmacopœia.

Then in the case of aconite two mistakes occur, the minimum quantity of ♏v. is too large, for we have all seen the remarkable power of drop doses frequently repeated in checking various inflammatory conditions, and the maximum is also too large, 15 minims being far from a safe prescription in the case of most adults who have over-passed middle life. We have already seen that tincture of digitalis is restricted to ʒss., and we know that the successful treatment of delirium tremens by means of ℥ss. doses is now one of the commonplaces of medicine; and Dr. Anstie draws special attention to the insufficient quantity ʒi. of tincture of hyoscyamus allowed, telling us that ℥ss. is often required in asylum practice.

It must be within the experience of all to have prescribed pot. bromide in larger doses than 30 grains, and the absurdity of restricting iodide of potassium to 10 grains is almost too patent to require further remark. We all know that some of the most brilliant successes of medicine and surgery have been secured by the administration of 20, 30, 40, and even 60 grains.

Coming next to ipecacuanha, we find that, although its properties as an expectorant and emetic are recognised, no mention is made of its specific anti-dysenteric powers; and a dispenser would therefore probably consider it his duty to cut down a prescription in which doses of from ʒss. to ʒj. of the powdered root were ordered to be taken every two hours.

The tonic dose of sulphate of zinc is put down at from one to two grains, although it is well known that more than double this quantity is often well borne in chorea; and 30 minims is considered the maximum quantity of tincture of ferri perch., although in erysipelas and acute rheumatism ʒj. is often administered with benefit. The minimum doses of liquor of morphia, hydrate of strychnia, and tincture of nux vomica, are all placed too high at ♏x. gr. $\frac{1}{30}$, and ♏x. respectively; and the researches of Ringer have shown that we derive little benefit from the use of tincture of lobelia in asthma, unless we carry the dose beyond that laid down by authority at ♏30. Now, although these are undoubtedly the principal instances in which the Pharmacopœia is hopelessly at variance with the modern development of therapeutical science, there are many other examples which I will not weary you by detailing.

Enough, I hope, has been said to convince you of the necessity for some alteration in the rules laid down for our guidance in the matter of dosage. The experienced medical man of course orders his drugs according to the dictates of his own personally acquired knowledge, and often arrives at practical conclusions, for which we might look in vain in our text-books; but the beginner feels himself hampered by authority, and the dispenser has his hands tied. On his counter lies the Pharmacopœia for constant reference, and, although not originally endowed with absolutely official powers, it has had that character forced upon it, and must abide the consequences. In any court of justice, in any professional controversy, its decision may at any time be invoked; and we have a right, therefore, to expect that we shall not be misled when its influence is thus brought to bear upon us. Not only should the scale of doses be at once raised, but pains might be taken to indicate the largest quantities which may be, or have been, given with impunity. And the important question of minimum doses, on the principle which is so rapidly gaining ground, of the advantages to be derived from keeping the system saturated with a drug, prescribing it—that is, in minute quantities—at such short intervals that its influence on nervous or vascular function shall not be temporarily arrested by elimination, might also receive attention. In short, without converting the Pharmacopœia into a treatise on therapeutics, the attention of those who have so ably superintended its construction might be called to the propriety and even necessity for some alterations in the rules which it lays down for the prescribing of drugs. To this it may of course be objected that therapeutics is not in a sufficiently settled state, nor is medical opinion on the subject sufficiently unanimous, to justify any authoritative utterance on these points, and in particular the question of frequent divided doses has not yet been fully worked out. This objection was foreseen by Anstie, and it remains with you to decide whether something might not be done by us to carry out his suggestions, and thus most effectually prove our reverence for the memory of one who was ever foremost in advocating the cause of truth and progress. Let me quote the concluding words of his paper: "No greater service could be performed by the colleges or great medical societies than the formation of a committee of competent men for the special investigation of this question of dosage, for it is a subject which is only in its infancy, and the best knowledge which exists about it is undoubtedly confined to a very small section of the medical profession." Now, although it may be true that the *best* knowledge of dosage has not yet become the common property of the profession, there is no doubt that enough is known to justify a pretty complete official classification; and as the expense of frequent reprints of the British Pharmacopœia would be considerable, I venture to suggest the occasional publication of a posological table in the form of addendum, which might give effect to recent investigation, and supply the not unnatural wish which generally prevails, for trustworthy and authoritative information on the subject of maximum and minimum doses.

Before leaving the subject of the British Pharmacopœia, I may lay before you another anomaly connected with dosage, which has been more especially exposed by Mr. J. A. Cope, of Derby, in the *Pharmaceutical Journal* for April 17th, 1875.

He there points out how inconsistently the doses of various drugs vary according to their preparations. Thus, the quantity of tincture of ergot sanctioned by official authority is equivalent to 13½ grains of the powder, whereas that of the powder or liquid extract corresponds to 3 grains. Thirty minims of tincture of digitalis equals three grains of the dried leaves, whereas the ordinary dose of the infusion is only equivalent in value to 1½ grain.

Aloes varies in its different preparations from three grains to eight grains; of tincture of belladonna, ♏30 equals gr. j. of leaf, whereas gr. j. of the extract corresponds to ʒj. of the tincture, whilst nux vomica supplies a still more glaring example, seeing that ♏20 of the tincture corresponds to two grains of the powdered seeds, whereas of the extract two grains equals ʒss. It is difficult to see that these curious discrepancies have been made with any deliberate intention, and the inference is therefore fair that they have merely crept in from some carelessness or want of method on the part of the constructors of our national Pharmacopœia.' Whilst fully acknowledging the great benefits which have resulted from the pharmaceutical fusion of Scotland and Ireland, and the establishment of one national standard in the United Kingdom, it appears to me to be the duty of everyone to make such suggestions for the improvement of future editions as may seem founded on reason; and it has been a great gratification to me to bring a subject before you which specially engaged Anstie's attention, and to the due furtherance of which he was about to summon all the resources of his energetic intellect when his career was unhappily brought to a close. There can be no doubt, however, that caution is a safe side on which to err, and the restrictions under which our chemists and druggists are now placed are our great safeguards against accident and disaster; and we may congratulate ourselves on this all the more when we see the looseness which prevails in homœopathic pharmacy. We all know of the change which has recently taken place in the principles of this class of practitioners, and how the feeble dilutions of former years have been replaced by excessively strong tinctures. This change, however, has not been followed by any alteration in the laws regulating prescribing; and we accordingly find these deadly poisons dealt out freely to all comers without restriction, and compounded by druggists who are not necessarily possessed of any legal qualification. In a letter from "A Chemist," published in the *Pharmaceutical Journal* for June 29th, 1872, this point is strongly urged, and the anomaly exposed of permitting homœopathic druggists to evade the Pharmacy Act by practising their trade without registration or examination, and making up patent and poisonous medicines under the same conditions as when their operations were confined to sugar globules and distilled water.

That many of the modern homœopathic preparations are of dangerous strength has long been generally known, but to Dr. George Johnson is undoubtedly due the credit of bringing the matter home to us more directly. It is unnecessary for me to do more than remind you of his paper read before the Clinical Society, and which, supplemented by additional cases, is printed in vol. vii. of their *Transactions*. Dr. Johnson brings forward five cases, occurring in his own practice and in that of others, in which unpleasant and even dangerous symptoms, consisting of epileptiform convulsions, violent pain in the head and spine, nausea, stupor, &c., were caused by doses of the concentrated homœopathic solution of camphor, ranging from eight 2-minim doses up to twenty 25-minims, and a teaspoonful. In some of these entire recovery only very slowly took place, and various distressing nervous sensations were experienced in the case of a young lady for fully eight months, after a single dose of 25 drops. As a contribution to our knowledge of the actions of camphor on the healthy organism, the interest of these cases is much enhanced by the accurate way in which the various poisonous influences on brain and spinal cord correspond with the account given of the physiological action of this drug as deduced from experiment by Nothnagel. From an analysis obtained by Dr. Johnson, it would appear that this homœopathic solution of camphor contains one ounce to one and a quarter ounce of spirit, being stronger than the spt. camph. of the B. P., in the proportion of 7⅕ to 1; and, considering the freedom with which it is used in domestic medicine, Dr. Johnson has undoubtedly done good service by directing attention to its dangerous properties.

Feeling desirous of obtaining some approximate idea of the strength of the strong or mother homœopathic tinctures, I procured some of the tinc. aconiti, buying it over the counter at a chemist's shop without any difficulty, and made the following experiments:—At six minutes to three o'clock on Wednesday, January 19th, I injected five minims beneath the skin of a full-grown rabbit, and no effect being produced in five minutes I injected 15 minims more. In three minutes, violent convulsive movements of the diaphragm set in, with attempts at vomiting, and a peculiar fighting motion of the fore-paws as though trying to remove some obstruction about the mouth. A paralysed condition of the hind-legs followed, and death ensued, after a paroxysm of general convulsions, in exactly five minutes after the injection was performed. Ten minims proved fatal to a rather smaller animal in twenty-five minutes, the symptoms following exactly the same sequence and regularity. I find that 90 drops, dropped from the ordinary homœopathic drachm tincture bottle, correspond to 60 minims; and we therefore see that 15 drops is sufficient to kill a rabbit.

I have no doubt that even a smaller quantity would also have caused death, but being unwilling to multiply these experiments, I lay before you my present evidence, which seems enough to show that we have here a potent and dangerous poison, sold publicly to any one, and doubtless recklessly used in domestic medicine.

So far as I can make out, the mother tincture of homœopathic practice about equals in strength our liniment of aconite, which we all know to be a very much stronger preparation than the tincture of the British Pharmacopœia.

Coming now to tincture of belladonna, I have found on personal experiment that ♏x. of the homœopathic mother tincture taken at night caused some dryness of the throat, with disturbed sleep and much dreaming; and three more doses at intervals throughout the next day were sufficient to keep up the same parched feeling about the mouth and throat, with an uncomfortable sensation of dryness and irritation over the skin generally.

This is therefore evidently much stronger than our own tincture, which may usually be given in doses of from 15 to 20 minims, without the induction of physiological symptoms until the drug has been persevered with for some time. I have once seen ten doses of 10 minims cause some discomfort to a delicate young woman; but, on the other hand, I have now a girl of fourteen under treatment for incontinence of urine who is taking with marked benefit ʒii. ss. thrice daily. An unpleasant dryness and thickness about the throat always precedes dilatation of the pupil as the first indication that the remedy is beginning to exert its constitutional influence, and the early development of these effects when experimenting on myself shows how strong the preparation in question really is. I have not made observations on any of the other vegetable mother tinctures, but find from the Pharmacopœia that they are as a rule constructed on the uniform principle of one part of the juice of the plant to one of spirit, a strength which must render their use most dangerous, unless under strict medical supervision.

As a fair sample of the metallic preparations of the homœopathic armamentarium, I have selected arsenicum and mercurius, both of which remedies are in extensive use; and being unable to obtain an accurate idea of their strength from the pages of the now antiquated Pharmacopœia, I applied to my friend Mr. Barker, who most kindly placed at my disposal the services of Mr. Brownen, the able analytical chemist of Messrs. Savory and Moore. This gentleman has favoured me with the following careful report on the amount of the active principle contained in the first potency of arsenicum and mercurius:—

Analysis of Homœopathic Solutions.—Two solutions were received for analysis.

Solution I. was labelled "Arsenicum Alb. Poison. Dose for an adult, one to five drops." This solution was examined for arsenic and arsenicum, as arsenious acid was found.

The menstruum was water, and it had a slight alkaline reaction.

The arsenic was estimated by volumetric and gravimetric methods with the following results:—

		Volumetric.	Gravimetric.
Arsenious acid . .	grs.	·994	·893
Water	grs.	100·000	100·000

Or, reduced to fluid measures, one grain of arsenious acid in 102 minims of water. The corresponding solutions in the British Pharmacopœia are each one grain in arsenious acid in 120 minims of solvent.

Analysis II. labelled "Mercurius Cor. Poison. Dose for an adult, one to five drops," &c.

This solution contained the amido-bichloride of mercury, dissolved in alcohol.

The solution was estimated volumetrically as regards its chlorine, and colorimetrically as regards mercury.

These operations gave the following results:—

Mercuric chloride	grs.	·75
Water	grs.	100·000

or about one grain in two fluid drachms, and about four times the strength of the solution of perchloride of mercury as ordered in the British Pharmacopœia.

Both preparations were therefore strong solutions of potent substances, and were rightfully labelled "Poison," as they were within the scope of the Poisons' Act.

Although we have seen that the precaution was taken to label these preparations, "Poison," I am not sure in how far they were freely sold, but in any case we find the ground cut from under the feet of the homœopathists, one of whose great boasts used to be that no mercury or injurious metallic substance was ever prescribed by any of their practitioners. Now, I feel quite conscious that the slight sketch just given of the strength of some of the principal homœopathic tinctures cannot be looked upon as much more than suggestive, and as a text for the remarks of others. I have not ventured to say one word upon, nor do I know anything of, the principles which regulate those who practise homœopathy. It is pretty evident, however, that to dose diseases solely by their names, and to prescribe infinitesimal dilutions, has not proved sufficiently captivating to the enquiring intellect of this sceptical age, and that, in order to sustain the principle of subdivision, excessive concentration still enables the practitioner to prescribe a minute quantity of his remedy. The strong tinctures, which, as we have seen, are freely vended to all comers, have luckily not yet been taken into very general domestic use, or one might anticipate a long series of tragical events, before which even Dr. Johnson's interesting cases would hide their diminished heads. The dangers to be anticipated would seem to me to be fourfold:

1. The free purchasing of these poisons with homicidal or suicidal intent.

2. The rash confidence engendered in the minds of old-fashioned homœopathists regarding the weakness of their drugs and the confidence with which they may be used. A mistake of this sort being made with aconite would not be likely to occur to the same patient a second time.

3. The great probability of the mother tincture being mistaken for some of the dilutions, and recommended right and left in the usual free-and-easy way by amateurs. This might also be serious.

4. And finally, the possibility of the strong homœopathic tincture being mistaken for, and used instead of, our weaker preparations; and in a competition of this nature, we, the allopathists, who are supposed to ruin the constitutions of our patients with enormous quantities of deleterious substances, must undoubtedly yield the palm of potency of dosage to those practitioners, the very essence of whose professional existence was formerly believed to depend on the magical effect of their infinitesimal dilutions.

It is fortunate that those amateurs who delight to dabble in drops and globules use as a rule the third dilutions, which are in all probability powerless for good or evil. I have never been able to get any physiological effects from personally testing any of these tinctures, and Mr. Brownen has been unable to obtain the slightest indication of the presence of even the smallest trace of arsenic or mercury in the attenuations of that strength. But we must remember that the potent mother tinctures are quite open to the public, that anyone may go in and buy any quantity for any purpose, and that they are invariably prescribed in the most effective way, by small and very frequently repeated doses. Such being the case I feel that I require no justification for having brought the case forward for your consideration and discussion.

INCONTINENCE OF URINE.

BY HORATIO BRENCHLEY, M.R.C.S.

It would be difficult, I imagine, to point out a form of disease more intractable in its treatment than incontinence of urine in the young. And there can be scarcely a hospital or dispensary surgeon who has not been baffled again and again in his management of such cases, or has not had to regret how little they respond to remedies. The unfortunate sufferers lead a life of misery; their night's rest is broken by their endeavours to keep awake; and they are subjected to the sneers and scoffs of their schoolfellows, who will scarcely speak to, or associate with them.

This is so commonly the case, that the temper and disposition of many a lad has been permanently injured by such treatment; and if this holds good with regard to young lads, how much more wretched it must be when young girls suffer from this affliction; and, indeed, it is to young girls especially that the treatment I advocate is adapted. A remedy, therefore, that can in any way relieve such a state of matters, requires no apology for being brought before the profession.

Belladonna, iron, and bromide of potassium, are some of the remedies that have been vaunted and tried in these cases, but I cannot say I have ever seen very much good done with them; and yet in most of the patients who come before us for treatment, iron seems to be generally called for, so that whatever form of treatment is pursued, iron should, I think, form one ingredient; and I have recently met with so much success with the following combination of ergot and iron, that I am inclined to write it down for the use of those who are willing to try it:—

℞ Tinct. Ergotæ, ♏x.
Tinct. Ferri Perchlorid, ♏v.
Spt. Chloroformi, ♏v.
Infus Quassiæ ad ℥j. ter die sumend.

I trust it may meet with a reasonable amount of success; I can only say it has proved beneficial in several recent cases.

ON THE INFLUENCE OF SEA-VOYAGES ON THE HUMAN BODY, AND THEIR VALUE IN THE TREATMENT OF CONSUMPTION.

BY C. B. FABER, M.D.,

Late Res. Med. Officer to the German Hospital, London.

(*Continued from* p. 220.)

II.—ON SEA-LIFE IN ITS SANITARY ASPECTS FOR SAILORS AND FOR PASSENGERS, PARTICULARLY INVALIDS.

THE sanitary conditions under which a man has to live are made up of three components—firstly, the climate; secondly, the physical condition of the dwelling-place; and thirdly, the social condition in which he is living. After having described the sea-climate, it therefore remains for us to consider the physical and social condition of that floating microcosm, a ship on high ocean.

Certainly great progress has been made within the last decades towards lessening the horrors of the sea and of sea-life. Voyages have been considerably shortened by the application of Maury's Sailing Directions, by the construction of fast-sailing clipper ships, and especially by the employment of steam. They have at the same time been rendered safer by a knowledge of the law of storms, and by the construction of iron ships. The use of iron in place of wood has also contributed towards the amelioration of the sanitary condition of ships. An iron ship generally is drier than a wooden one; it keeps out that source of contamination of the air, the bilge-water, much better, and if there is any, its smell is less offensive, and its quality not so bad as in a wooden vessel. An iron ship can be also kept clean, more easily and effectually, than a wooden one. But, on the other hand, the latter has, in a sanitary view, certain advantages over the former. A construction of iron excludes that natural ventilation which is going on through the sides of a wooden ship, as far as they are above water. Under a tropical sun, the body of an iron ship is heated like a furnace, and retains the heat during part of the night.

The improvements just mentioned have not, however, been made so much with a view to rendering the lives of those on board more healthy and safe, as for getting higher profits out of the shipping trade. Merchant-ships are still being built on the principle of getting as much room as possible for cargo, and allowing as little room, air, and light as the law will allow to men. Much ingenuity has been bestowed, and oftentimes wasted, upon systems of artificial ventilation for dwellings on land where natural ventilation may be quite sufficient; but the ventilation of ships—a question of grave sanitary importance to the large class of seafaring people, and, it must be confessed, of particular difficulty—remains still in a most primitive state.

The diet-table of first-class passengers certainly has improved, having profited especially by the large extension and the improved methods of the art of preserving all kinds of animal and vegetable food; but the diet of the common sailors is still, to use a strong, but not too strong, expression, abominable. In this respect our convicts are probably better provided for than they. It is my belief that a medical Samuel Plimsoll, furnished with careful and extensive statistics—for which, of course, my own experience is far too short—would be able to bring forward sufficient evidence to convince the public and the legislature of this country of the fact that, to provide a wholesome dwelling and good food for the sailor at sea is no less urgent a necessity than was the Unseaworthy Ships Bill. To this end, every medical man in the large seaports of this country, who is in the practice of sending patients to sea, might contribute by visiting the docks from time to time, and visiting the class of ships he sends his patients out in, so as not only to be well acquainted with the sort of accommodation they have to expect, but also to be able to point out and direct medical and public attention to certain defects and nuisances.

This might prove a wholesome stimulus to shipowners, and lead them to compete in the construction of ships on improved sanitary principles.

In now entering into the details of the sanitary condition of ships on a long sea-voyage, my remarks refer specially to that class of vessels by which alone an invalid can think of going—viz., to first-class passenger (not emigrant) ships.

The most important point, especially to a consumptive patient, is fresh and pure *air*. No doubt a sea-voyage affords more opportunities of getting, or, I may even say, compulsion to get it, than any other climatic treatment. The cabins or the saloon (cuddy) of a ship have no great attractions to keep one, who can possibly go on deck, down below for any length of time during the day. So one spends almost the whole day, and also a good part of the night, in the open air, and will face every sort of weather on the open deck, not only in the fine and hot weather of the tropical and sub-tropical latitudes, but even in less genial latitudes. What an important point this is in reference to the value of sea-voyages in the treatment of consumption will be shown later on.

One might expect the air on the open deck—at least, on board a sailing-vessel—to be as pure as possible. There are, however, even here, sources of contamination. There is, on the main deck, a small steam-engine for distilling water and other purposes, which sometimes sends puffs of smoke and coal-dust over the quarter-deck. Whether the smoke in a steamer ought to prejudice us against recommending this class of vessels to invalids, will be discussed as a principal point in the question—Steamer or Sailing-vessel? Then there are placed about on the deck, sheep-pens, pigsties, a cow-pen, and a large number of hen-coops, the latter even all round the poop, under the very nose of our invalid. The smell they send forth makes itself perceptible often in a very offensive way, particularly in the damp heat of the tropics, where those hen-coops may become regular hot-beds of bacteria, micrococci, and other productions so famous of late for their (alleged) disease-producing properties. The air in the ship itself is of very different qualities in different parts.

The frigate and clipper-built sailing-vessels and steamers in the Australian trade generally have not a flush deck, but have an upper forecastle and a poop, from which one looks down on the main-deck. This arrangement has, as regards ventilation, a decided advantage over a ship with a flush deck, as I myself have had sufficient opportunity of experiencing, my first ship, a mail steamer, being of the latter; my last one, a sailing-vessel, of the former description. Through the two doors, opening from the poop and the forecastle, on a level with the main-deck and through the companion-ladder of the poop and the hatchway of the forecastle an ample ventilation can be effected. The forecastle, however, which is occupied by the crew, is low, dark, and overcrowded. The poop is not only several feet higher, but has also much more room, air, and light. In the centre is the saloon, with skylights above, which can be opened. The cabins, which run along the saloon, have, on the one hand, a large square window, on the other, they open into the saloon, not only by a sliding door, but also by venetians and above at the ceiling, so that free communication of air is possible all through the poop.

There are usually two or three bunks in a cabin. An arrangement is common in Australian ships, which I have not seen anywhere else, and which certainly is not to be recommended for imitation. Each cabin has a water-closet of its own in one corner. Remembering the free communication of air all through the poop, the consequences of this arrangement as affecting the whole community of the first-class passengers can well be imagined without further explanation. Apart from this nuisance, the ventilation of the poop, in and upon which our invalid is to spend his days and nights, may be considered sufficient, provided that the weather is fine and the seat not rough. But when stormy, rainy, or cold weather, and a rough sea, render it necessary to shut up all those ways of ventilation, when even the daylight is almost shut out from the cabins by the putting on of the "dead-lights," with their dim "bull's-eye," and this for days, and even weeks together, then the air in such close quarters must very soon become far from good, and this state of things must tell visibly upon invalids, especially if they are not strong and hardy enough to face that sort of weather on deck and have some fresh air and exercise. *One* *week of such confinement may undo the good that a month of fine weather at sea has effected.*

In the 'tween deck, where not only the second and third, but in many ships also part of the first-class passengers are quartered, this close and stifling state of the air is almost perpetual, for the sea is rarely so smooth, and wind and weather so steady, as to allow of the opening of the port-scuttles. If it were not for the open hatchways—through which, however, besides the air, in rainy and rough weather, a varying amount of water comes down below—the air here would be stifling, especially if it happens to be contaminated not only by the emanations of the people, but also by those of the cargo (wool, hides, &c.) and of the bilge-water. Take with this a tropical heat, which greatly intensifies all those emanations, and you will agree with me, that a long sea-voyage is for a 'tween-deck passenger neither a wholesome nor pleasant thing.

There are two contrivances for ventilating the lower parts of the ship. The first, for the benefit of the 'tween-deck passengers, consists in wind-sails, a spread of canvas hung up over the main-deck by ropes, and contracting downwards into a cylindric canal, which is fixed over the hatchway. In the calms of the tropics, when ventilation is especially necessary, wind-sails are of course of no avail whatever. The other contrivance, for bringing air down to the cargo, consists in big hollow iron cylinders, the caps of which are taken off in the tropics during the daytime.

I have never found a ship where there was sufficient air and light in the lower parts. Speaking of the air within the ship, it remains shortly to mention the *disinfectants* used on board. The floors (decks) are regularly scrubbed with sea-water and chloride of lime, or some other disinfecting powder. The smell arising therefrom may prove rather trying to an invalid with a weak chest. The water-closets are washed out with the same powders, or with crude carbolic acid. Perfect cleanliness and order prevail in a first-class ship.

The *water* on board a ship of the class I have in view is partly supplied from the land, partly from the sea by way of distillation. Steam-engines have been introduced for this purpose, chiefly with the view of sparing more room for the cargo, but they are also a great improvement in a sanitary point of view as they prevent that distressing calamity at sea, the running short of fresh water. When taking in their supply of water for the voyage, sailing-vessels without those engines appear to discount in advance the supply they expect to get from the tropical rains. If these fail, or if the voyage lasts longer than anticipated, then that calamity arises. Descriptions of it, which I have heard not only from sailors, but also from a retired general, who experienced it even in H.M. troop-ships, make one appreciate the stills more thoroughly.

The distilled water, which is consumed during the greater part of the voyage, is almost chemically pure, and not at all so tasteless as might be expected. Perhaps it is the contact with the iron tanks, in which all fresh water is kept, and the contact with the air in the process of filtering it from the flakes of rust from the tanks, which gives it some taste.

The natural fresh water keeps very well in those tanks, even during a long voyage. That in the wooden tanks formerly in use it often turned bad, and became disgusting to the nose and the palate, I can easily believe. I remember, during a fearfully hot time at Melbourne, the thermometer standing as high as 110° F. in the shade, how delicious and cool the London water tasted, of which about sixty gallons were fortunately found left in one of the tanks. In that time of extreme scarcity of water, our discovery was soon known among the shipping, and people came far for the sole purpose of asking for a drink of water, and wanted even to buy it.

The quantity of fresh-water allowed to each passenger and sailor for drinking, cooking, and cleaning purposes, is three quarts a day. First-class passengers, however, are not restricted to this measure. They can, within reasonable limits, usually get as much water as they want. Rain-water may be collected by sailors and passengers of the lower classes. After a heavy tropical rain, there is usually a general washing-day on the main-deck and forecastle. Most ships have bath-rooms, where cold sea-water baths may be taken. Besides this, in the tropics, gentlemen passengers may have a morning bath on the poop-deck, consisting of several buckets of salt-water poured over their heads by sailors.

Of *drinks*, the crew and third-class passengers receive water, tea, coffee, and lime-juice. Only on extraordinary occasions, or after very hard work, is a ration of rum or gin served out to the crew by order of the captain. This teetotalism at sea is no doubt advantageous to the shipowner, but it is questionable whether it is good for the sailor, and has not a good deal to do with his excessive intemperance in port. I may mention here the often-observed fact, that most people require, or at least may take with impunity, more and stronger stimulants at sea than on land. Adult second-class passengers are allowed one pint of porter daily, and one bottle of wine a week. First-class passengers have to pay for all their drinks except water. There are the usual sorts of spirits, wines, and beer.

Lime-juice deserves particular mention here, because it is generally believed to be *the* anti-scorbutic. But, as Dr. Garrod has shown, it is not the want of organic acids, but of potash in the food by which scurvy is caused. The blood in scurvy is according to him deficient in potassic salts. Lime-juice contains a good deal of potash, yet, as it cannot well be taken in quantities large enough for this purpose, it does not secure a sufficient supply of those salts to the organism. This is shown by the outbreak of scurvy in ships where lime-juice had strictly been used. Fresh potatoes are much more effectual as an anti-scorbutic than lime-juice, as they, according to Dr. Garrod's valuable analysis, contain about as much again of carbonate of potash, and may, moreover, be taken in much larger quantities. A still more effectual anti-scorbutic is, in my opinion, *Liebig's extract of meat*. As to the nature and the mode of action of this extract there is a wide-spread misconception, by which essentially and directly nourishing qualities are ascribed to it. Nothing could refute this notion more effectually, than the fact, that of two animals, say dogs, of which the one is put to absolute starvation, whilst nothing but Liebig's extract of meat is given to the other, it is the latter that dies soonest.

The essential qualities of Liebig's extract rest on its principal constituent, potassic salts. These not only form a most important constituent of our organism, especially of the bioplasm by which it is built up and continually restored, but they moreover accelerate this process. They act as a stimulant to the tissue-change (*stoffwechsel*). In this circumstance the explanation of the experiments mentioned just now is given.

Scurvy consists not only in a deficiency of potassic salts, but also in an extreme sluggishness of the "stoffwechsel," which takes the form of a specific dyscrasia. In the treatment as well as in the prophylaxis of this constitutional disorder two requirements have to be fulfilled. Liebig's extract of meat, as has been shown, does this. Potato food may supply the organism with a sufficient quantity of carbonate of potash; lime-juice does even that insufficiently. Therefore that food may prove sufficient to prevent a full out-break of scurvy; but it cannot prevent altogether that lowering and debilitating effect which the sailor's life and, above everything else, the sailor's food has on the organism, and the features of which will be given in a later paper "On the Physiology and Pathology of Sea-Life." Certainly the stimulating properties of the potato, if it has any, cannot be compared with those of Liebig's extract of meat, which, if combined with sufficient nourishing food, enables an organism reduced by acute disease quickly to repair its losses, thereby shortening the time of convalescence, and on the other hand prevents a healthy organism from being deteriorated by food otherwise unfit to keep it in lively function and vigorous action. Besides, a soup simply made of Liebig's extract of meat, hot water, and some bread, biscuit, or rice boiled in it makes a palatable dish, which is more than can be said of most things in the sailor's bill of fare as it is at present.

After what has been said I need scarcely add that I should consider it a great and material improvement if Liebig's extract of meat were added to that bill of fare *by law*. One or two teaspoonfuls a day per man might be sufficient, and the cost of that improvement would be trifling compared with the good it would do not only to the sailor but also to the employer, by keeping up the vigour and the efficiency of the former even during the longest voyage.

At the same time, however, I would not by any means recommend the omission of potatoes and lime-juice from the dietary regulations for seamen. For the former a vindication is superfluous. Who would like to be without potatoes in one shape or other for a week only?

Lime-juice makes a pleasant and wholesome drink in hot climates, refreshing and cooling and at the same time acting on most people as a mild aperient. I have seen babies taking the breast affected by it too much in this way, so that the mother had to give up taking lime-juice. A good many people take it too concentrated, then it not only affects the bowels, but also the stomach, impeding digestion and spoiling the appetite. I have found the right dose to be just enough lime-juice to cover the bottom of a tumbler which should be filled up with water.

Passing from the fluid to the solid food I have to repeat that the seaman's bill of fare is not a particularly inviting one. Salt beef or pork every day of the week but one, when there is preserved Australian meat; suet, rice, potatoes twice a week, biscuits, a paste interspersed with some raisins, called pudding, those are the principal things that make it up, and on this the seaman has to subsist for months together without any change. Is it to be wondered at that after some time at sea even old seamen, whose stomachs have been accustomed to this diet for twenty or thirty years, find it trying not only to their taste and digestion, but also to their constitution?

The salt beef and pork are kept in wooden casks, but do not always keep well. I remember in the second part of one of my voyages, when such a cask was opened on the main-deck, all the passengers on the poop used to run towards the stern as if for their lives holding pocket-handkerchiefs or smelling-bottles to their noses.

With regard to the *preserved Australian meat* I must confess that before I went to sea I shared the prejudice against it so common in this country. But soon I came really to like it and to appreciate it as a beneficial change from mutton, pork, and poultry, although these come from the live-stock with which the ship is provided for the first-class passengers all through the voyage. In hot weather, however, the savoury and nourishing jelly with which it is covered melts away and the juice it contains oozes out, so that the meat remains neither palatable, tender, nor nourishing. Apart from this, the only fault I have found with the preserved Australian meat is, that it is very often too much cooked in the process of preserving. Since the plan has failed of sending whole ship-loads of fresh meat packed in ice over from Australia, the meat-preserving companies ought to pay particular attention to cooking the meat as little as possible.

The common biscuits, as the seaman gets them, are much more difficult to digest than bread for two reasons: first, they contain much less water, which makes them less easily soluble in the stomach; secondly, they are of a rougher material than our usual bread. In masticating a biscuit of this description it requires a large amount of saliva to get the several particles moist and soft enough to be swallowed without their irritating the throat. It would be an interesting point to make out whether, according to the theory of natural selection, this circumstance, together with the habit of chewing tobacco common among sailors, would lead in the course of a number of generations to an inherited enlargement of the salivary glands.

The improvements in the diet regulation for seamen, which I consider necessary from a sanitary view, are the following:—

Preserved meat	3	times a	week.
Fresh potatoes	4	„	„
Cabbage, turnips, peas or beans	3	„	„
Fresh bread	3	„	„
Liebig's extract of meat . . .			

This list is of course not to be taken as a complete diet scale, but as a supplement to and a substitute for the existing regulations which certainly cannot be called extravagant.

The scale of victualling for the third-class or intermediate passengers is not much above that of the seamen. That of the second-class is better and more varied, but there is no fresh meat in it. The cuddy-table is well supplied with everything that can be had at sea. There is fresh meat every day of the different kinds already mentioned, preserved meat, fish, and shell-fish, vegetables of every description so well preserved that one might take them for fresh, fresh potatoes, fresh fruit at the beginning only, but preserved fruit of every kind made up in puddings and tarts, jams, and fresh bread every day all through the voyage.

But notwithstanding this variety, and the good quality of the several articles, which at first sight might well make one forget that he is on high ocean and not in a good class English hotel, sooner or later a certain monotony makes itself felt not only to the palate, but to the system in general, all the more that as a rule ship cooks pay little attention to getting things up nicely and temptingly. More and more one misses and longs for those "nice little things" which cannot be well had at sea, but which have become almost necessities in our epicurean age. Then even fresh eggs, fresh butter, fish and oysters, fresh fruit, lettuce, and watercress appear to one who has not seen land for a good long time as the greatest possible delicacies which he would readily pay anything for if he only could get them.

The ship cow, which, side by side with the stewardess and the "experienced surgeon," figures daily in the shipping advertisements, supplies fresh milk, which, however, as may be expected, is not of very good quality and does not go far, being for the most part required for infants and sick people. So Swiss preserved milk has to come in as an important supplement.

The same enemy, which stealthily comes over the dinner-table and takes away the relish from the meals and to a greater or less extent the appetite, Monotony, sooner or later creeps also into society. The quietness of sea-life is, from a therapeutic point of view, generally considered one of the principal points in favour of it. This is certainly true to a certain degree and extent. But it must be admitted, on the other hand, that, when it makes itself felt as monotony, its beneficial influence becomes doubtful. The period after which one grows weary of sea-life of course varies considerably according to the individuality of a person, his or her relations and liking to the ship's company, especially fellow-passengers, according to the sort of accommodation and life on board, to the state of the weather and sea, the expected duration of the voyage. To fix the period a sea-voyage ought to last for invalids, I reserve for a later paper.

If one is lucky and at the same time able to "rough it," and accommodate himself with good will and temper to a number of people with whom he is brought into such close, continual, and inevitable contact, he may find a nice and pretty homogeneous company, which will soon form into a sort of large family-circle and combine their several resources to keep that enemy, *ennui*, away as long as possible. The ways, however, in which this is done are often not at all suited for invalids, and I frequently had to remind them to abstain from taking part in theatricals; running, jumping, climbing, boxing and similar matches. In the southern oceans angling and flying lines for those large sea-birds of the Petrel family, with which those regions abound, diverts the unoccupied energies for many an hour.

But he must be of a dull mind who is satisfied for months with nothing more than conversation, novel-reading, sketching, games, and sports such as those just mentioned. A more active and intellectual mind will long for better occupation to spend part of the day in, so as to give some relish to the idleness of passenger sea-life. A professional man will not be at a loss in this respect; he will even find that he has much less time to himself than he may wish for.

In disagreeable society a long sea-voyage is a hard trial, if one cannot make himself independent of it, as far as the close social contact on board a ship will allow it, in the way just hinted. A sea-voyage, therefore, also in a social regard, involves a good deal of risk, a fact which is particularly important to an invalid, the more so, if he be without a companion from home.

The sailor's life at sea is very different from that of a passenger. Instead of monotony its principal characteristic is irregularity. Days of quiet and idleness, when scarcely a rope has to be touched or a mast to be climbed, as in the trade-winds, may be followed by days and nights of constant alarm, when the men scarcely get a wink of sleep, and besides being hard-worked may be cold and wet through all the time. In this respect the South Pacific Ocean, particularly off Cape Horn, and the English Channel are notorious.

But the irregularity of the sailor's life is still greater on land than at sea, and moreover the contrast between sea and land life very great. Of the consequences I shall speak in a later paper.

Here it only remains to mention a class of "hands on board," the lot of which, in my opinion, is the least enviable of all; I mean the engineers and firemen in steamers. Their work, deep below in the hold of an iron ship, in a temperature which I am assured in the tropics often reaches 120° F., involuntarily reminded me of slave-labours. Seeing them come up from the engine-room streaming with perspiration and their generally pale and haggard faces covered with soot, one cannot help the impression, that their wages, high as they are compared with those of the seamen, are hardly earned.

Clinic of the Month.

Treatment of Ringworm.—Mr. Erasmus Wilson considers that a really healthy child cannot have ringworm. Therefore, where tinea is found to exist, the practitioner should set himself the task of improving the strength of the patient. Ringworm is usually most rife at the growing period of life, it is rare in infancy, and ceases altogether after puberty; so that it is at the growing period, when the greatest demands are made on the nutritive power of the individual, that the disease prevails. It is at this period that demands are also made on the vital powers for education, and children are removed from the care and indulgences of home to the less agreeable duties of school. There can be no doubt that there may be poorly nourished children at home as well as at school, and hence ringworm may prevail in the family at home as well as in the family at school. As a prophylactic against ringworm, and also as a remedy for its cure, Mr. Wilson would begin by enforcing a generous and nutritious diet, meat in some form three times a day, puddings made of flour and suet and varied in flavour and taste, and a little good beer. The quantity must be regulated by the appetite of the child. There need be no excess, but every meal should be as nearly as possible equally nutritive. Adults deem it necessary to have three nutritive meals in the day, whilst the children of the family are often put off with two, possibly because they are little; but they are in reality the germs of the future great, and have an abundant employment for their nourishment in building up a healthy structure for the coming man. If they are deprived of the wherewithal, the coming man, and all that springs from him in the future, will be weak and feeble, and as time wears on will develop those diseases which are known to be the consequence of a feeble constitution. The diet of children we often find reduced to the miserable standard of bread and butter and milk and water for breakfast; for dinner, meat and milk and rice puddings with water; and in the evening, bread and butter and milk and water. No better method could be devised for engendering ringworm, scrofula, phthisis, and cancer. It is quite true that in some instances children will not thrive even on the best selected diet, but in these cases it is perfectly surprising what may be accomplished by a few drops of arsenical solution added to the food. It is with this object, after securing a good and substantial diet as a preliminary to the treatment of ringworm, that Mr. Wilson is in the habit of prescribing ferro-arsenical mixture in doses representing two minims of Fowler's Solution with each meal three times a day. This treatment he declares to be infallible, and suitable to every case. Where ringworm prevails in public institutions or in considerable aggregations of children, it is customary to lay the blame on contagion, whereas it would be more consistent with truth to admit that the surroundings of the invalids are not favourable to the promotion and maintenance of general health. Cod-liver oil, which is sometimes of great value in tinea as well as in favus, is probably nothing more than a means of nutrition, and may take its place by the side of diet. And arsenic is a useful and indeed necessary remedy. (*Medical Examiner*, April 6, 1876.)

Cancer of the Stomach.—In a lecture on this subject in Volkmann's *Klinische Vorträge*, Prof. Will. Ebstein observes that the stomach can claim the unenviable privilege of being more frequently attacked by cancer than any other internal organ, various authorities giving 35-45 as the proportion of gastric to all other forms of cancer, whilst only from 15-18·5 per cent. are of uterine origin. It appears to be rare in Egypt and in Vera Cruz, though gastric catarrh is common in both places, discountenancing the ordinary view that the latter predisposes to the former. All classes of society and both sexes are almost equally liable to it. Three-fourths of the cases occur after the age of fifty. The causes of it are unknown, though it may be due to accident or injury, and result from hereditary influence. A large proportion of cases commence at the pyloric orifice; and it is almost always primary. Waldeyer brings forward evidence that the cancer of the stomach develops from the peptic glands and the cells of the pyloric glands. As soon as these have penetrated beyond the muscular layer of the mucous membrane they begin to proliferate most vigorously in the vascular submucous tissues. The connective tissue does not remain passive, but a growth of small cells, with an abundant development of new vessels, commences at an early period between the follicles of the glands. The symptoms of gastric cancer are divisible into two groups. First, those resulting from the disease affecting the stomach; and secondly, those consequent upon the disease being of a cancerous nature. Some of the symptoms, however, as pain and trophic disturbance are common to both conditions.

The stenosis of the pylorus arrests the onward movement of the contents of the stomach, and leads to the dilatation of the organ, and the degree of this distension may be ascertained by percussion after the patient has drunk some effervescing fluid. The longer the disease lasts the longer the interval between two attacks of vomiting, but the larger the quantity vomited on each occasion. The vomited matter is usually of the consistence of thick soup, offensive, sour from containing acetic and butyric acid, which again result from the decomposition of the fats and carbohydrates under the influence of sarcina ventriculi and torulæ. Where the vomiting follows immediately on the ingestion of food it is due, in some instances, to spasmodic contractions of the œsophagus. The vomiting of blood, either pure or mixed with food and in variable quantity, is a very well marked and diagnostic symptom. In regard to treatment, gastric juice, which Schiff has recommended as a caustic in some forms of external cancer, has been shown by Virchow to be of but slight efficacy when the stomach itself is the seat of the diseased action. Von Bencke has suggested the employment of non-nitrogenous food, with such stimulants as wine and tea, and his method is perhaps worth a trial. At present, treatment must be essentially directed to the symptoms that arise. In stenosis of the pylorus, with considerable dilatation of the stomach, the use of the stomach-pump has in some cases afforded remarkable relief by unburdening the organ of the masses of stagnant and fermenting food which it contained; while, however, some patients are able to tolerate the instrument day after day on account of the after comfort which they derive from it, others are so much exhausted by the operation that it is impossible to continue it. In this latter class of cases the patients do best under the use of remedies which arrest fermentation in the contents of the stomach. Prof. Ebstein has found 10-15 drops of benzine given in mucilage, and still better an aqueous solution of carbolic acid, ·1-300, in doses of a tablespoonful five or six times a day, of extreme value. They arrested the frightful eructations of sour smelling gas which rob the patients of their sleep. In stenosis of the cardia, as soon as the diagnosis is tolerably clear, all attempts at passing a sound into the stomach must of course be abandoned, otherwise the consequences may be most serious. All that can then be done is to nourish the patient by enemata, according to Leube's method, as long as possible. In dealing with a cancerous stomach, gastrotomy has nothing to recommend it. The symptoms of chronic catarrh, which are sometimes the only ones present, may be treated by giving small quantities of food at frequent intervals, followed by two or three tablespoonfuls of very dilute hydrochloric acid (0·4 per cent.). In such cases benefit is sometimes derived from small doses of iodide of iron in combination with bitters, the iodine apparently acting through its antiseptic and anti-putrescent qualities. (*Med. Times and Gazette,* Feb. 19, 1876.)

Is Skating injurious to Women?—Dr. J. H. Aveling states that he has for some years had a young lady under his charge suffering from scanty and irregular menstruation, spots on the face, constipation, languor, and general debility. About six months since her father erected a private rink at the back of his house, upon which she has skated every day, sometimes fatiguing herself extremely. The result has been most satisfactory. All the functions have become normal, the spots have disappeared, and the general health and strength have become quite satisfactory. Dr. Aveling has watched the effects of skating upon himself as well as on women, and believes this exercise to be not only compatible with the health of women, but such as to be best fitted for their peculiar organization. The motion is easy and flowing, no sudden exertion is required, and, except in case of accident, the body is not subject to rough jolting or concussion. (*Medical Times and Gazette,* Feb. 12, 1876.)

Domestic Measurement of Doses.—Dr. Farquharson finds that although the drop is usually held to be equal to a minim, it varies within very wide limits. The number of drops which were equal to 60 ♏ were 56 drops of distilled water; 113 of tinct. opii; 114 of tinct. digitalis; 100 of liq. morph. hydrochlor.; 80 of oxymel scillæ; 75 of syrup papaveris; 45 of glycerine; 114 of spt. ætheris nitrosi; 112 of tinct. camphoræ co.; 55 of acid sulph. dil.; 55 of ol. ricini; 110 of spt. terebinth.; 100 of ol. anisi. The shape of the bottle also influences the size of the drop, but not to such an extent as the nature of the fluid. Tablespoons vary in size, the largest examined containing eight, and the smallest five, drachms. As they are usually supposed to contain half-an-ounce, the doses of medicine measured by them are too large. The dessertspoon was found to contain from four to six drachms, and the teaspoon from eighty minims to two drachms. The wine glass was found to contain from two ounces and two drachms to three ounces and two drachms instead of two ounces, as is usually imagined. (*Brit. Med. Journ.*, Feb. 26.)

Extracts from British and Foreign Journals.

Iodide of Potassium in Syphilis.—Dr. Joseph R. Beck contributes a paper on the use of iodide of potassium in syphilis. He states that he has not made use of mercurials in the treatment of syphilis for a long time. Notwithstanding that secondary symptoms rapidly disappear when mercury is employed, he claims we have only succeeded in such cases in masking the disease, and that after a time tertiary symptoms are sure to follow. To use his words, "Every case of secondary syphilis which has been successfully (?) treated by mercurials will, as surely as the sun rises, reappear as tertiary syphilis if the patient lives long enough." Syphilis, either secondary or tertiary, cannot be radically cured in a few weeks or months, but requires a year or more of careful treatment. Dr. Beck implies that secondary syphilis can be so successfully treated by the iodide of potassium as never to recur in its tertiary form. For the cure of syphilis by the iodide of potassium very large doses must be employed. He prescribes seven drachms of the iodide, and four drachms of the ammonio-citrate of iron in an eight ounce mixture, and gives a tablespoonful before each meal. Fowler's solution of arsenic (one drachm three times a day of an eight ounce mixture containing three drachms of the solution) is also prescribed, and if anæmia be present, one grain of quinine and two of pulvis ferri are given every three hours; also cod-liver oil with fluid extract of valerian. If ulcerations be present, he brushes over them three or four times a day a solution of hydrate of chloral, one part to two of distilled water. Such are the first prescriptions. In each of the succeeding prescriptions he adds one drachm of the iodide of potassium to the formula, and the dose of Fowler's solution is augmented by one half of a drop. "The iodide should then be carried up drachm after drachm in strength with each successive prescription, until we reach twenty or twenty-five drachms' strength to the formula, or as much as is necessary, taking care to order the increase made only with each succeeding fresh prescription." The arsenic is increased until a dose of five drops of Fowler's solution is attained, when it is finally omitted as an adjuvant.

The iodide, however, is increased till symptoms of decided iodism are induced, when its administration is stopped for a week; it is then resumed for a week, and if iodism shortly again ensues, Dr. Beck confidently dismisses his patient "perfectly and permanently cured, with the poison of syphilis and that of mercury for ever eradicated from the system." (*Boston Med. and Surg. Journal*, No. xxi., vol. xcii.; *Philadelphia Medical Times*, March 13, 1875.)

The Treatment of Diarrhœa in Young Children.—Diarrhœa, says the writer of a paper read before the Boston Society for Medical Observation, particularly in those under two years of age, and in the summer season, usually begins very insidiously, commencing from a slight chill or a meal of improper food; a protracted and high temperature in a large city also enters largely into the ætiology of the affection. The bowels first become slightly relaxed, but the purging is neither severe nor of long continuance; it speedily ceases and the child appears to have recovered. The bowels, however, do not return to a healthy condition, and the complaint is then that the bowels are confined; perhaps two or three days later the child will have two or three large sour, pasty-looking dejections, more or less slimy and passed with efforts of straining; the stools may then become more frequent and be occasionally streaked with blood; and some abdominal tenderness may be present. The presence of undigested food in the dejections of a young child, especially if that child exhibits evident marks of deficient nutrition is an indication that the diet is not suitable and that it should be changed; but the accurate adaptation of diet is by no means an easy task in such cases. It is not very uncommon to find cases where milk, whether diluted with water, or thickened with isinglass, or with farinaceous food, cannot be taken. Liebig's farinaceous food or soup is tolerably well borne in many cases, and it is occasionally advisable to try it. Flour for want of a ferment is incapable of being digested by the stomach of infants. In using Liebig's soup this want is supplied by the diastase of the malt. When cream (or excellent milk) can be obtained for infants, it is beyond all comparison the best food for them, and no addition need be made to it except water and sugar in the following proportion:—

Child in good health.	Cream. parts	Boiling water. parts	Milk Sugar. parts
1 week old	1	11	25
2 weeks old	1	10	25
3–4 weeks old	1	8	25
1–2 months old	1	7	25
3–4 months old	1	6	25
4–6 months old	1	5	25
8–10 months old	1	3–4	25

Bicarbonate of potash may be added if it turn acid on the stomach. The alkali should be combined with an aromatic, for this class of remedies is of great value in all those cases of abdominal derangement when flatulence, pain, and spasm resulting from vitiated secretions and undigested food are present to increase the discomfort of the patient; such phenomena are usually rapidly relieved by the use of these agents and the employment of anise, cinnamon, carraway seed, or even tincture of capsicum in minute doses will be found of advantage in combination with other remedies. If the child be so ill that he is only capable of taking a small quantity at a time, it may be well to give beef-tea in conjunction with the milk; a teaspoonful may be given occasionally, and its digestion will be aided by adding a pinch of saccharated pepsine to each spoonful. In some cases the child should be fed very frequently, but it is better that the child should be hungry than that his stomach should be overloaded. Stimulants may be required, and in fact they are always required when the fontanelle becomes much depressed. If the stools are loose and frequent two or three grains of subnitrate of bismuth may be given, and if there be much straining a drop of tinct. opii or a little Dover's Powder may be added. Enemata of the fluid extract of krameria are occasionally useful. If an aperient be required there is nothing better than the tinct. rhei. aquos. of the German Pharmacopœia, which is made as follows:—Rhubarb, 100 parts; borax, 10 parts; potass. carb., 10 parts. Pour on these of boiling water 850 parts; set aside for a quarter of an hour and add alcohol 100 parts; then add cinnamon-water 140 parts. As soon as convalescence is fairly established iron and cod-liver oil may be given. The child should be weighed once a week, and great attention should be paid to the action of the skin. (*Saint Louis Med. and Surg. Journal*, No. 9, 1875.)

Solution of Bromine as Dressing.—In the Bellevue Hospital, New York, a valuable stimulating application is obtained by dissolving two drachms of bromine in a pint of water. The use of bromine in a dilute form is rather a novel method, and it seems to offer some advantages. (*New York Med. Journ.*, Feb., p. 172.)

Treatment of Dysentery with Salicylic Acid.—In St. Francis's Hospital, New York, cases of acute dysentery have been treated satisfactorily by means of doses of thirty grains of salicylic acid administered three times a day. The only objection to the remedy appears to be a slight irritation of the stomach, but this is not sufficient to cause emesis. (*New York Med. Journ.*, Feb., p. 173.)

Department of Public Health.

THE CHOLERA IN SYRIA AND THE PROPHYLAXIS OF CHOLERA IN EUROPE.[1]

BY MAX VON PETTENKOFER,

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THE unexpected appearance of cholera in the year 1875 in Syria, the violence of its outbreak in some places, its absence in others, and lastly, the narrow geographical limits to which it has been confined, have excited great interest both in the East and in the West. The facts announced by consuls, by newspapers, and the *Gazette Médicale d'Orient* give a very incomplete picture of the entire Syrian epidemic. It would be very acceptable if an exact, and at the same time a full, description of its distribution could be obtained from an intelligent observer. The little, however, that we at present know in regard to the cholera in Hama, in Damascus, and Beyrut, as well as the condition of Lebanon, leads us to compare it with the outbursts of cholera in Europe, and suggests the advisability of subjecting the usual measures against the spread of cholera to trial, and to inquire whether it be practicable to arrest its progress by the methods hitherto employed, or whether it is advisable to adopt other plans and neglect the old ones.

In order to demonstrate that the cholera in Syria, and the preventive measures against its spread, accord with those which we employ in Europe, I shall in the first place communicate a few striking incidents of the Syrian epidemic, and then proceed to consider what can be learnt, and what conclusions can be drawn from them, so that we may know what medical, as well as what political measures are or are not likely to prove of service in this important sanatory question. I hope to be able to show that the time has come when we may endeavour, in addition to trade and railway politics, to introduce some amount of sanatory knowledge, to resist the evils accompanying increased intercourse, without interfering with its advantages. The reader will excuse me if I take this opportunity of reproducing many facts which may not be exactly new, and even some which I have already stated word for word, in order to accomplish my purpose.

I. THE CHOLERA IN 1875 IN SYRIA.

After Syria had been severely visited by cholera in the summer of 1865, the land had rest until this year, when Hama, a town containing about 30,000 inhabitants, with a garrison, situated on the Orontes, between Aleppo and Damascus, was first attacked in April. No cholera existed far and wide around Hama, nor in Syria, when it suddenly broke out in the military hospital of this town.

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[1] The author has sent us this paper for the *Practitioner*, but he has also published it in German in the *Zeitschrift für Biologie*, Band xii.

The outbreak in Hama was quite unexpected. This town is so isolated that it was hoped the disease could be limited, and that the place of the outbreak could be surrounded by a military cordon : an idea that had immediately to be given up, since it was found that the disease, in spite of all the precautions taken, spread, and it became evident that the cordon was incapable of effecting its purpose. It would be a subject of great interest to have an accurate account of this cordon, both as to what was intended to be done, and what was actually effected.

Damascus, Aleppo, Antioch, Latakia, Tiberias, Tripoli, Saida, Hauran, Jebleh, Beyrut had cholera, and sporadic cases occurred also in various places in Lebanon. The disease attained its acme in the month of August, Damascus being chiefly affected. It lasted here from the end of June to the middle of August, and was brought into it by soldiers who were employed in forming the cordon around Hama. It proved fatal to 9,319 persons, including 8,894 Mohammedans, 278 Christians, and 147 Jews, which was more than 6 per cent. of the entire population, amounting to 150,000.

Other Syrian towns fared better, the disease being less violent than in the epidemic of 1865. In Beyrut, for example, the first death occurred on the 27th July, the patient being a woman who had been brought into it the day before sick of the cholera from Damascus. In Beyrut the disease was very mild, attaining its greatest intensity from the 5th to the 10th August, during which period, on an average, 6 persons died of it per diem, the greatest number amounting to 12. The population of this town is about 60,000.

From the 27th July to the 1st September, there died :—

Of Mohammedans	71
Viz., 22 males, 30 women, 19 children	
Of Christians	65
Viz., 32 males, 25 women, 8 children	
Of Jews	7
Viz., 3 males, 4 women	
Total	143

Between the 1st and 15th of September ten more persons died of cholera.

It was believed that three circumstances essentially contributed to the mildness with which the disease appeared in Beyrut.

1. The exodus of three-fourths of the population to the mountains.

2. The precautions taken by the Mutaserriff for the maintenance of cleanliness in the town, and for the procurement of the necessary medical assistance.

3. The exceptionally low temperature that prevailed during the period that the city was visited by the disease. But if it be true that 45,000 out of 60,000 of the population had fled, and the 153 deaths are to be distributed amongst the remaining 15,000, the epidemic was tolerably violent, and in a few weeks more than 1 per cent. must have died of those that remained.

The governor of Beyrut was greatly aided in his efforts to preserve the health of the town by the Ottoman Bank, the German and French consulates, the imperial deaconesses, and the French *sœurs de charité*. The deaconesses were supplied with the means of providing food for 450 poor people for two months, partly by the German consulate, and partly by means of voluntary contributions. This materially alleviated the horrors of the plague, and deserves all praise.

But now how came the cholera in this year to Hama, which we must regard as having been the centre of infection for all Syria?

In the first place it might be supposed that it was introduced by some affected person from without, especially by soldiers, since a number of recruits had shortly before arrived, and the epidemic first exhibited itself in the military hospital. But careful examination, the results of which are given in the *Gazette Médicale d'Orient* and the *Journale de Smyrna* of the 28th August, 1875, soon showed that all the recruits had come from Albania, *viâ* Beyrut and Damascus, and that these, neither during the voyage to Asia Minor nor during the march on land, had touched at any place suspected of cholera nor come in contact with persons from one.

It was then said, the cholera had remained in Hama since the winter, and had been introduced there by a Persian caravan of pilgrims, which came from Bagdad. It was, however, conclusively shown that this caravan consisted of people who were perfectly well, who exhibited no single case of disease during their temporary stay, and had departed as healthy as they came. Moreover no cholera existed either in Bagdad, or in Hedschas, nor in Yemen, before it broke out in Hama.

A few Syrian physicians are inclined to admit that the cholera originated autochthonously in Hama, or developed from germs which had remained latent from 1865, had then become contagious, and had spread by infection (Ansteckung). It is not surprising that such views should arise in Syria, since they are held by many living in the centres of European knowledge and medical science.

Hama was already known as a place possessing conditions highly favourable to the development of cholera, from which it had suffered severely in the year 1865. An eye-witness described the place in the following terms. (See No. 34 of the *Levant Herald*, of 25th August, 1865.)

"Hama is pre-eminent for dirt amongst all the towns and places of Syria, with the exception of Tiberias and the villages of Lebanon. It stinks and stiffens with dirt. Impure as is the water of the Orontes as it reaches it, it leaves it in a still more impure condition. In addition to all waste products, heaps of accumulated impurity, layers of dead dogs and other remains of former generations remain unmoved in the majesty of filth. When the cholera appeared in Hama it was indeed at home and amongst its friends and allies. I know nothing that approximates in dirt to Hama, except a village of Lebanon."

If the filth of Hama is to be regarded as the soil for the development of cholera, it is impossible to understand why the flight to Lebanon by the population of Beyrut, where the impurities are as great as in Hama, proved so effective. In spite of its filth, Lebanon on this occasion, with the exception of a few isolated, and for the most part introduced cases, remained free from cholera. It would hence appear that dirt alone without some additional factor is incapable of causing a cholera-epidemic.

Is it possible that the measures for isolation and disinfection were so admirably carried out in Lebanon that every introduced cholera germ was prevented from reaching the local filth, or that the cholera cases which reached them with those who fled from the plains and valleys could produce no further infection? The correspondent of the *Levant Herald* expresses a very decided opinion on this point; he says:—

"The quarantine system in Lebanon, as carried out under the authority of Rustem Pasha, is deserving of profound study. The Pasha can scarcely be made answerable for the details of the scheme, but in its entirety it well illustrates the impossibility of an effective local land quarantine. The roads conducting to these Lebanon villages, which vary in distance from one to eight or nine hours' journey from Beyrut, are very tortuous, and to some of them, which are very widely separated, six or eight different paths may lead. The sheiks of the villages are required, by order of the Pasha, to subject every person coming from Beyrut to a quarantine of six days. Furniture, beds, etc., are likewise kept for the same period in quarantine; but flour, rice, coffee, sugar, corn, etc., are emptied from the sacks of the muleteers upon mats, and after being exposed to the air for a short time, are again placed in the old sacks, and brought into the village, whilst the muleteers are still kept in quarantine.

"The chief route from Beyrut into Lebanon, as far as to the principal seat of the Pasha, passes through a town, at the outskirts of which the quarantine tent is erected close to the street. Several families are kept together under the slight linen covering, and others sleep beneath the olive-trees in the vicinity. Now a Mussulman, whom we may term A, arrives from Beyrut with his family and occupies a tent. He is, speaking in terms of quarantine, in the course of six days pure, that is to say he can go into the town; but on the third day Mussulman B comes from Beyrut with his family and occupies the same or the adjoining tent, and in this family cholera suddenly bursts out; but after six days A is still allowed to leave as pure, although he may have been for three days in immediate contact with cholera patients and their belongings.

"Another characteristic trait of a Lebanon quarantine, as actually carried out, is that it is maintained within the boundaries of the village, but is provided neither with physicians nor with medicines. Whosoever is attacked in this barbarous place with cholera must simply die and poison the air around him. Should a local doctor try to visit a patient in this aerial prison, he is forbidden to return into the village.

"The alternation between the heat of the town and the cold night air of the mountains requires that extraordinary precautions should be taken, especially for women and children; but in many instances even a canvas tent is wanting—a vine, a fig, or olive-tree is all the protection against the burning heat of the midday sun and the frost of midnight. A few days previously Dr. P. of Beyrut was called from Damascus into Lebanon to a rich Hebrew family. The household consisted of forty people, of whom twenty-five were sick, suffering from the cold and fever of the open quarantine air of Lebanon. In order to pass from one village to another it is necessary to obtain a pass signed by the sheik of the village, stating from what town and at what time the bearer came. If the good old sheiks of Lebanon were quite inaccessible to a bribe all would be well; but where is the man who would make an affidavit that the necessary signature to a pass could not be purchased from anyone of the sheiks for two or five francs, in which pass it is stated that Said or Omar, even if he have just arrived from Beyrut, has come from such and such a village, and is a 'pure' individual.

"Another characteristic trait is that those who fly from the cholera at Beyrut often pass in their way to Lebanon through a score of villages before they settle in one, and thus the cholera may frequently penetrate into the mountains. It is in the last place only before they finally settle down that they are inclosed for six days in a mount of olives and exposed to the air of fever.

"The untenability of the system is obvious, but the Pasha must yield to the pressure of the terrified fugitives from cholera, who, after they have themselves fortunately escaped, tremble lest the cholera should penetrate to them in their mountain asylum.

"Similar measures were in operation in the year 1865. The muleteers who came from Beyrut were kept in quarantine in the hills outside the town, but as soon as it was dark they crept into their towns and slept in their houses. It is now just the same. The only circumstance which can be adduced in favour of this system of quarantine is that it calms the nerves of the timid and increases the general confidence in the security afforded by the mountains. An effective quarantine is impossible; piastres open every one of these linen doors to Lebanon.

"But who can describe the harass and annoyance experienced by merchants, physicians, and others who have their families in the mountains, and who, owing to the necessities of business or practice, are compelled to remain during the week in the town! The physicians and the consuls indeed cannot be kept out, but must be allowed to pass through, and as, finally, all well-to-do people have the same claim, the whole thing is an absurdity."

This account, given by an eye-witness, is certainly caustic, but it gives the impression of truth. It would indeed be surprising if the quarantine in Lebanon did not find its upholders, for however bad it may be, the results are in its favour; Lebanon has remained on this occasion also free from the local epidemic. There must have been some reason for this, and he who can think of no other must be satisfied with the quarantine. It is consequently in Syria just as with the disinfection of sewage in Europe, every place in danger of cholera must now be disinfected. If a town remains free from cholera, the disinfection, even if no better than the quarantine in Lebanon, has preserved the town from cholera. If it be attacked, it is said that the disinfection has not been able to effect its purpose, or the objection is raised that in these cases the means have not been properly applied, and the place has been but imperfectly disinfected. But in the case of the military cordon around Hama, since it was fruitless, something must have failed which under much worse circumstances did not take place in Lebanon, for the cholera, in spite of the cordon, spread from Hama, whilst the quarantine preserved Lebanon, although it was so bad that an easy explanation could have been afforded if, in spite of the quarantine, the cholera had spread epidemically in the mountains. How long will our sanatory police arrangements rest upon such weak—I might almost say such puerile—arguments, before it attains its full growth and acquires a solid footing? How long will the unreasonable pecuniary sacrifices continue that such fruitless theories leave in their wake?

We now proceed to consider another point which suggests itself from the contemplation of the cholera in Syria. Since the flight from cholera on these occasions proved so advantageous in Beyrut, we may naturally ask the question, whether this means should not also be tried in Europe?

Two causes essentially lead to an exodus from cholera; on the one hand, people naturally leave an infected place, and, secondly, they seek one which is not only not infectible, but which possesses an immunity from infection. It was on this second ground that people fled from Beyrut, not to the sea-coasts, but into the mountains. Towns which are far distant from a mountain or other immense district must not dream of this advantage; but for those towns and places which are in a position to obtain this benefit, which is no longer impossible even in India, especially in barracks and gaols, the experience gained in Beyrut is not very satisfactory. Another eye-witness, writing upon this point from Beyrut on 7th August, 1875, says:—"Jews, Mohammedans, and Christians have fled to the heights and spurs of Lebanon, till these mountain villages are crowded with strangers. Hundreds of thousands of piastres flow daily into the pockets of these poverty-stricken mountaineers in the form of extortionate rents, jobbing of mules, and the provision of food of the innumerable guests. Beyrut presents in its principal streets the same appearance as Pompeii. The dogs run about hungry, and can no longer find a bone. The night is dreaded by the lonely watchmen who are hired to protect forsaken houses. Burglaries are common, and in spite of all the watchfulness of the Mutaserriff the greater number of the better class of refugees find on their return in autumn to Beyrut that their houses have been robbed of everything that is valuable and portable."

It is obvious that a civilised European town can scarcely adopt this plan, since the consequences would in every instance be worse than actual warfare. Nothing else can be done by the majority than to remain on the scene of the disease; those who fly from cholera will always constitute a small minority. What is to be done for those who neither can nor will fly? Make a cordon, disinfect, burn sulphur here and there, clear out a spring, confiscate sour cherries and cucumbers, avoid fat sausages, clear away accumulations of manure, have no dance-music, but visit only churches and theatres, &c., &c.; all of which proceedings are perfectly useless. But that under such circumstances the fatalism of a Moslem no longer maintains itself has been shown by the cadi (the judge of Beyrut) when he tramped in the midst of the crowd of refugees to the mountains. An Arabic journal, *Thumrat-el-Fannoan*, which is edited by a talented young Mohammedan in Beyrut, had a long article upon "Law and Faith" from the pen of the learned Mussulman Achmed Effendi Achdab, in which believers were reprimanded for flying from the cholera; but at the conclusion, in order not to appear too severe, he gives an anecdote of the Caliph Omar, who when he once refused to enter some place were the plague raged, was asked: "Wilt thou escape God's law?" he answered "Yes! I will *go from* God's law *to* God's law." And thus to-day crowds of the Mussulman inhabitants fly in order to await God's law, not in Beyrut, but rather in Lebanon, like the great and strictly religious Caliph Omar.

II. Chief Factors in the Spread of Cholera.

In regard to the last question, respecting which—speaking generally—the public at large and those more immediately interested are chiefly concerned, namely, whether the cholera of 1875 in Syria, and the numerous observations and researches to which the devastating ravages of cholera in the world generally have given rise and supplied the materials, offer any substantial and reliable facts by which the occurrence of the disease may be prevented, or if not, be so restricted in its extension as to render flight from it unnecessary—in order, I say, to answer this practical question it is necessary to recapitulate and to repeat to some extent what has already been frequently said. Three principal factors which play a prominent part in the extension of cholera are at length thoroughly recognised:—1, The influence of intercourse with places where the disease is raging; 2, The various behaviour of individuals to the specific cause of cholera, which proceeds from cholera districts and is distributed by intercourse, for it is not everyone that comes into contact with it that is equally liable, some being easily and others with difficulty affected, whilst the greater number are not attacked at all—a peculiarity that is termed individual disposition or idiosyncrasy, under which head we may also include the successful therapeutic treatment of cholera patients; and 3, The influence which the surrounding conditions exercise, since there are places in which the cholera when once introduced regularly increases, and others in which it never attains its full development; and this is generally regarded as due to the influence of the locality, amongst which atmospheric and temporary influences are to be included.

(*a*) *Influence of Intercourse.*—From a prophylactic point of view the first thing to be done is to make experiments which may counteract the influence of human intercourse. As it is impossible either for the sake of cholera, or even of life, to dispense entirely with personal intercourse, attempts have been made to remove the adhering cholera-germs by cordons, quarantines, and disinfectants. But if the results of these efforts be critically considered, no benefit can be shown to have resulted from them, nor can any hope be entertained that better results will be obtained in the future. In no instance has it been possible to protect even small and isolated points like Gibraltar, Malta, and Gozo from the entrance of cholera when the epidemic existed in the vicinity. They have been in all instances affected when their time had come. It may indeed be said that the precautionary measures hitherto practised have been defective, and ought to be improved, but no one suggests how. For example, no quarantine can be better arranged and enforced than it was in 1865 in Malta, and which was maintained from the very moment that the outbreak of cholera in Alexandria was telegraphed to Malta, from the 14th June to the 31st July, and was only given up after the disease, in spite of quarantine, had spread as usual over the whole island. The practice of quarantine is conducted under the most unfavourable conditions, for in the first place we do not know the agent which it is intended to] suppress or destroy; secondly we know nothing as to what it is conveyed by, or to which it can cleave; and lastly, human intercourse is so many-sided and complex a thing that it can never be so far controlled as is requisite to exclude all adherent germs. It is smuggled in at every toll-gate, and every blockade is broken; but since a single germ, introduced at a favourable time, can supply the fruit or the seeds for an entire country, the uselessness of these precautionary measures is obvious.

In addition, we know nothing as to when intercourse brings the cholera-germ with it, and we know nothing also as to how long after being introduced it may remain latent before it begins to exert its influence at some point. When and how the cholera-germs came to Hama in order to produce an outbreak of the disease in April, 1875, is as completely unknown as when and how it came in 1873 to Heilbronn, Speier, or Munich. In Munich, however, evidence has been obtained that the germ may at least remain quiescent without causing an epidemic, and can then unexpectedly suddenly produce one. The division of the last Munich epidemic into a summer and a winter epidemic is one of the most important and interesting ætiological facts. If we can admit that in 1873 the cholera-germ was brought from Vienna to Munich, and then burst forth at a definite time, we must so much the more admit that in Munich during the summer epidemic, at the beginning of August, it came from the Neuhaüsergasse to the valley, and from the Karlsplatz to the Gärtnerplatz. A few isolated cases already occurred in the summer, indicating that the cholera was to be regarded as having entered into these parts of the town, but Thal and Gärtnerplatz, as well as generally the south-east and low-lying part of Munich, were first affected by the disease in an epidemic manner in the winter. The introduced germ must thus have remained latent for four or five months before all the conditions were established that were required to develop an epidemic. But if so long a period may elapse between the introduction of a germ and its epidemic action, who can say whether or not the cholera-germ had been brought to Hama by intercourse at the same time as it entered Damascus and Beyrut, and only subsequently developed, and that the cordon around Hama, and the quarantine in Beyrut, were not established at far too late a period to prevent its introduction?

Some are now inclined to admit an autochthonous origin of the cholera poison in Hama; but if this be admitted for Hama, why should it not be admitted for Damascus? Perhaps because the epidemic in Hama was preceded by no other outbreak in the neighbourhood or in Syria, whilst in Damascus the epidemic developed itself a few weeks later than in Hama.

To admit both the autochthonous origin and accidental introduction together, just as may be most convenient, is not only an arbitrary proceeding, but also an untenable one, and it is impossible to coincide with the view advanced in the *Gazette Médicale d'Orient*, "that in order to explain the origin of the disease in Hama and its spread, we must admit that overcrowding, dirt, and poverty have generated the choleraic disease, to which the specific element is superadded, and that the contagion associates itself with the primitive affection, and henceforward plays the most important part." Nor can we admit, "that from this point of view the epidemic in Hama represents a fusion of the views of the localists and contagionists." Such a fusion would be the resolution of both theories in nothing.

(*b.*) *Influence of Individual Disposition.*—The second factor in the extension of cholera, namely, individual disposition, can be but imperfectly met by general precautionary measures. Could we deprive all men of a disposition to cholera we should be as little affected by the disease as the various domestic animals, none of which exhibit any disposition to cholera, and any means which can effect this is equivalent to absolute interdiction of intercourse with cholera-infected places; unfortunately, however, our knowledge of individual disposition is still very imperfect. We know, indeed, that those under five years and those over forty years of age are much more frequently attacked by severe cholera than those between ten and twenty years, who usually suffer from it in the form of slight diarrhœa—that the poor and debilitated are more liable to suffer than the rich and strong; but one cannot during the prevalence of an epidemic of cholera make all men between ten and twenty years old, nor the poor rich, nor the weak strong, but they must essentially remain in their ordinary condition at the time of the outbreak of cholera. States of the body that have long existed cannot be suddenly changed. Moreover the local treatment of the patients has not made any material progress, since half of all cholera cases still die. It is known that such severe cholera cases, one half of which prove fatal, are preceded frequently for some time by diarrhœa, but that in many instances diarrhœa does not pass into cholera; it unquestionably often happens that timely medical aid is capable of preventing many a case of diarrhœa from passing into cholera, but in a broad point of view this is not effected. Very recently the hypothesis has been advanced,[1] that the entire disposition to cholera is nothing more than an intestinal catarrh, and that the point of an epidemic of cholera could be broken by the domiciliary investigation and treatment of diarrhœa. Without undervaluing this suggestion, which is no doubt worthy of being carried out, it cannot be denied that from the facts in our possession no very great hope can be built upon it. What may be accomplished by medical visits from house to house has been frequently tried, and especially in all barracks during the last long cholera epidemic in Munich, to an extent that is scarcely possible in private houses. Every male ward was carefully investigated for diarrhœa each day, every case of diarrhœa that could be discovered was immediately taken to the military hospital for appropriate treatment; but when ultimately a comparison was made between the mortality of the garrison and the civil population of Munich, not the slightest difference was discovered in favour of the soldiery, though subjected to so many strict and thoroughly carried out precautions. This is so much the more remarkable, since the military hospital both during the summer and during the winter epidemic was preserved from a house epidemic, and could not therefore be regarded as a centre of infection for other diseases, whilst the two civil hospitals did suffer from house epidemics, that on the left of the Isar having even two. In the prisons also the occurrence of diarrhœa during the period of the cholera epidemic was watched with great care, but here also no positive results were obtained. In the Laufen prison, amongst 522 prisoners there were 128 cases of cholera, 43 cholerines, and 126 of diarrhœa, with 83 fatal cases. In the House of Correction, Rebdorf, amongst 353 prisoners there were 34 cases of cholera, 31 cases of gastro-intestinal catarrh, and 50 cases of diarrhœa, with 22 deaths. To adopt expensive precautionary measures at the public cost it is necessary that something more than mere opinion should be entertained, and that they should possess some demonstrable sanatory value.

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[1] Dr. Freymuth, *Gibt es ein praktisches Schutzmittel gegen die Cholera Versuch der Rettung der Haus-zu-Haus-Besuche?* Berlin. Herm. Peters, 1875.

We do not, however, mean to say that no attempts should be made to discover means capable of diminishing the individual disposition to cholera to the greatest possible extent. If it were possible to find one of equal efficacy with vaccination, in the case of small-pox, it would be of the highest importance. For the prevention of small-pox isolation upon the whole was of little value, in fact of no greater value than in cholera, until a means was found which did not require exclusion of intercourse with variolous patients, but permitted this to take place freely whilst endeavouring by diminution of the personal predisposition to small-pox to diminish the power of the small-pox poison. Let us hope that some fortunate accident, like the experience of the milkmaids of Gloucestershire in regard to the protecting power of cow-pox, may also occur in the case of cholera and fall under the cognizance of a physician equally as acute and unbiassed as Dr. Jenner.

(c.) *Influence of Place and Time.*—It still remains to be considered whether the third factor in the extension of cholera, viz., locality, presents greater practical importance. Both old and recent researches have beyond all doubt established the fact that the essential point of an epidemic does not lie in the distribution of the cholera germ, but in the locality in which it falls. The germ is constantly being distributed by human intercourse in many ways from cholera districts, but in spite of free and unrestricted intercourse epidemic explosions rarely take place, and where they occur we are generally compelled from a comparison of the numerous places in which it does not occur, although it has been introduced, as well as from a careful consideration of all the circumstances, to admit local and temporary causes external to the patient as contributing to the disease. Whoever follows the extension of cholera from a centre of infection along the various modes of intercourse, and from single cases along wider circles, cannot overlook it; and, in opposition to the predominant views, it cannot be repeated too frequently that intercourse with cholera districts is at most accompanied by the danger of supplying the tinder or the match, but that the force of an epidemic which alone drives us to take sanatory measures, and which sporadic cases never do, depends on locally accumulated combustible material or powder, with which the mine must be charged if the spark is to exhibit a powerful action. Hence in a practical point of view the most important thing would be to discover the locally distributed powder in these mines in order that it may be gradually removed before the sparks begin to fly through the air, rather than to pursue the several sparks carried by the intermingled modes of intercourse, and to try to extinguish each of these before a mine under our feet or around us is exploded, throwing us and our extinguishing apparatus violently into the air. Everyone is aware that the burning match lighting upon a gun without powder is quite harmless, by which we frighten children only and can at most merely burn our fingers.

In a sanatory point of view it matters little how the germs of the disease are brought if it occasion no considerable impairment to the general health. The cholera germ is, I might almost say, fortunately connected in its action with a number of local and temperary conditions, and where these are not found, although it may enter by various modes, it does not produce any such injurious effects that would render it worth while to adopt general measures against its introduction, even if we were in possession of the means that could prevent it. The general distribution of the cholera germ is of less consequence than the place to which it is brought.

The accessory conditions which must act concomitantly are several in number, but it is only recently that a scientific and exact investigation of their nature has been earnestly undertaken, and on this account but few have been made out. One at least can be admitted, namely, that the soil, and the pollution of this by the sewage, plays an essential part, though we may know nothing of its action and connection with the cholera germ distributed by intercourse. This contamination of the soil is only one condition, and is not sufficient in itself; other conditions must concur, of which we at present know very little. This, however, does not prevent us from recognising and discussing it as an essential *one*.

The pollution of the soil with the sewage of the human household is only a part of the cholera locality, just as the carbon is only a constituent of gunpowder; with this constituent alone not the slightest explosive action can be obtained, though it is indispensable in the form of gunpowder; and so also with the other constituents. With sulphur and nitre alone no action can be produced, a right mixture of all three being requisite. The villages of Lebanon may thus have as much dirt or carbon as Hama, but it would appear that the necessary quantity of sulphur or nitre is not present, or that the whole powder may be too moist for the cholera sparks that proceed from Beyrut and Damascus to inflame.

Local cholera powder is not probably of such simple composition as gunpowder; but this simple illustration shows how essential is a definite mixture of its components in order to produce an explosion, and that at least one, two, three, are always necessary. Even in the perpetual home of cholera, in Lower Bengal, the conditions for an epidemic are not always present: sometimes the sulphur or the nitre, or both, fail for variable periods of time, although the dirt, the carbon, and the tinder or the match of a few cholera cases are always present. In many places all the necessary conditions are rarely present, whilst in some they are never present. Thus, for example, Lyons has up to the present time successfully withstood the numerous introductions of cholera germs, since 1830, from Marseilles and Paris, with a slight exception in the year 1854, where after antecedent abnormal drought, lasting for half a year, the two portions of the town lying low upon alluvial soil, and named Gillotière and Perache, were epidemically attacked by cholera, whilst the districts Croixrousse, Fourvière, St. Just, St. Foy, &c., which are situated on granitic elevations, remained on this occasion as free from cholera as did the whole of Lyons in 1849, although at the time there was an insurrection in Marseilles and Paris, cholera raged, and the town was beleaguered, conquered, and garrisoned by regiments who had brought the cholera with them. During cholera periods the better classes fled from Paris and Marseilles to Lyons, just as the inhabitants of Beyrut and Damascus fled to Lebanon, and with the same good result. Lyons has only once during the last forty years, and then only to a limited extent, afforded all the conditions requisite for the development of cholera, although dirt and facilities for the entrance of the poison have never been absent. The overcrowded artizan quarter of Croixrousse is perhaps as filthy as a Lebanon village, and I have myself visited houses there which might even be compared with those of Hama.

It is possible that we might discover other and perhaps still more definite local and periodical conditions that are supplied by the soil and its impurities which might be compared with the sulphur and nitre of gunpowder, if the subject had been more carefully investigated, and by the removal or prevention of which an epidemic explosion might be prevented; at present, however, it is sufficient to stick to the carbon, and to remove this everywhere as far as possible. If any process depend on a chain or series of essential causes it is certainly not necessary to break every link in the chain, but the rupture of one destroys the continuity of the whole. And thus, although there is no doubt that human intercourse is an essential link in the chain of causes leading to the distribution of cholera, it is unfortunately one of the most obscure, and in practice amongst the most awkward and most difficult to grasp and to abstract. It is impossible to give up all intercourse in order to protect ourselves from cholera, for the consequences would be much worse than an occasional cholera epidemic; independently of the circumstance that all efforts in this direction have hitherto proved fruitless, and will probably always remain so. Let us keep, therefore, at present to the soil.

III.—CHOLERA PROPHYLAXIS.

He who regards the purification and preservation of the purity of the ground upon which human habitations are built as the principal aim of practical activity against cholera epidemics, must first be clear as to what is or can be done in this direction, and whether what has been done has proved of service. Most people believe when they hear of the purification of the soil of a town or of a place that there is really nothing to be done, since it is impossible to give to any place another position and a different soil from that which it has obtained from nature; the essential conditions must be left just as they are, or have become in the course of time, the hand of man cannot materially alter this. It is of course impossible to change an alluvial soil into compact granite, or a chalk formation upon which the place is seated into sandstone; no other alteration can be made in the soil than to alter that which man has already changed, and modify it in a different or opposite direction. Up to the present time we have, for instance, frequently impregnated the soil with the sewage of our households, in future this shall no more be done, but this sewage shall be directed in another way, and shall be carried as far as possible from our dwellings; and as we have hitherto made the building ground for our houses inconceivably impure with all kinds of abominations, and allowed them to spread their mischievous influence around, we will now cleanse it with pure air and pure water, and we shall then soon drive out all dirt which has remained in it from ancient dirt, just as a cemetery becomes free from decomposing dead bodies without exhuming them as soon as we cease to make new burials.

Regular draining and abundant supplies of pure water, removal of all cesspools, and generally of all circumstances which up to the present time the soil of our houses and their immediate neighbourhood have generally provided with abundant nourishment for organic life, abolishing all interruptions to the flowing off of water from the surface and under it, which lead to such great variations in the degree of moisture contained by the soil, must be the end proposed.

It is unjustifiable in the supply of water to make a distinction between drinking water and water for general use, so far as the purity of the water is concerned. If we bring impure and germ-carrying water in great quantities into our dwellings and upon the surface of the soil, we certainly plant by this means frequently more injurious influences than if we drink a glass of such water, in so far as our stomach, owing to its peculiar juice, digests many kinds of infection which, if introduced directly, or through the medium of the lungs into the blood, act injuriously. The researches of Colin, for instance, have demonstrated that in the stomach even the poison of malignant pustule is disinfected and rendered harmless.

The requirements of the arrest of soil contamination, and the requirements for the purification of a soil impregnated with impurity must be satisfied, not only on account of cholera, but on other sanatory grounds, for contaminated soil plays a prominent and no longer to be misunderstood part, not only in cholera epidemics but in other very important diseases in a sanatory point of view, as for example, in abdominal typhus. Every town therefore which seriously sets itself to fulfil this object not only does something temporary in preventing cholera, but effects an important sanatory improvement of permanent value.

It can only be objected to this, that in every recognition of advantage and of the use of such a proceeding there are still great technical difficulties, that it is very expensive, and upon various grounds not always practicable.

The technical difficulties are now in great part happily overcome, and as increased attention is directed to them will be still further overcome. Even if it be impossible to prevent the entrance of all impurities into the soil, it can still be materially diminished. The problem is certainly a difficult one, and but few people have a clear idea of it.

For each person excretes in 1 year 11 cwt ...	74·8 lbs. of fæces, 941·6 lbs. of urine, 198 lbs. of kitchen refuse and house cleansing, 83 lbs. of ashes, with wood, firing, &c.
146 cwt	1,606 lbs. of water, using 20 litres per diem,

making yearly several loads for each person, which must be conveyed away, if it is not to accumulate and penetrate into the soil. As these numbers correspond to a mixed population of men, women, and children, and the body-weight of an average person can at highest only be taken at about 100 lbs., it is astonishing that the yearly weight of the urine alone surpasses ten times the whole weight of the person by whom it is discharged.

These numbers render it intelligible to everyone how incomplete are our present methods of removing sewage, and to what a high degree this must contribute to the contamination of our dwelling-places. Up to the present time under ordinary circumstances the soil must certainly absorb more than ninety per cent. Hereafter if it is impossible to remove everything, we must still endeavour to reduce the amount of impurities. As regards the cost, this is easily covered by the discharge of the excreta, which has often been done in epidemics without grumbling. If of the 60,000 inhabitants of Beyrut, of whom three-fourths fled to Lebanon, only 40,000 remained there for two months during the cholera epidemic, and each day only one shilling was paid for each person, the two months of cholera-flight alone cost twenty thousand pounds. If a complete purification of the soil is not everywhere practicable, this constitutes no reason why it should not be aimed at where it can be accomplished. It does no harm, but may be of much service, if only the towns, even the larger towns anticipated the lowlands. The towns are much larger centres of intercourse than the villages. On this account the towns are consequently affected at an earlier period by cholera than the adjoining country, and were there no cholera in the towns it could not have been introduced into the country. The larger a place and the more free its intercourse, by so much the more important is it that it should remain free from introduced epidemics.

But anxious minds inquire, Will the means proposed really help us? Do we not sometimes deceive ourselves? It is remarkable that those who put these questions conceive no measures to be doubtful as long as the danger is around them, and that nothing is too expensive if it can be immediately obtained and applied; but as soon as the danger has vanished again, these same people quickly become hypercritical, and everything is too expensive, though it may fulfil many other objects than that of merely banishing fear. Many believe themselves to be wise when they say this is not a question merely of the soil and its impurities, but there are a number of concomitant circumstances against which something may be done, and if all recommendations that are now pressed be followed, something no doubt can be accomplished in all directions, but upon the whole nothing. Instead of removing as far as possible one only of the essential constituents of the powder for a local epidemic, as, for example, the carbon, and thus rendering inoperative the remaining mixture of nitre and sulphur, they take a little carbon, and a little sulphur, and a little nitre away, but leave still powder enough on the whole to explode as soon as a spark enters from without.

The thing must be thoroughly done in one direction at least. If drainage be made and filth collects as before in the houses and in the cesspools, if the drains serve no other purpose than the removal of the rain and spring water, or if the canals are without proper levels and inclines, or without being flushed or without solid beds, they then can certainly not diminish the impurity of the soil, but may even tend to increase it. But where the work has been carried through with a thorough comprehension of its true object, it has everywhere hitherto exerted a favourable influence upon the local sanitation, and is especially effective as a preventive against cholera. The investigation made by Mr. John Simon in the Ninth Yearly Report, 1866, to the Privy Council, upon the diminution of mortality in twenty-four English towns after the completion of the so-called sanatory works, is in its essential results corroborated by all the facts hitherto known. The limited distribution and the small intensity of cholera in England in the year 1866, the non-participation of England since this time in the cholera epidemics of the neighbouring continent, must be regarded as a telling witness against the numerous and violent epidemics in the 30th, 40th, and 50th decades in England, that we are practically in the right path. In 1866 Lübeck, in opposition to its former behaviour, had only a few cholera cases, after a regular system of drainage had been carried out at the instigation of Dr. Cordes; and who would now doubt it should consider the behaviour of cholera in two German towns as recently as 1873, and which were formerly characterised by the violent and frequent cholera epidemics from which they suffered, I mean Halle, upon the Saale, and Dantzig. As often as the conditions for a cholera epidemic existed in the municipal district of Dantzig, the town of Dantzig was the headquarters of the disease, and on this occasion the cholera was as violent as formerly in several parts of the municipal district, and even extended to the very gates of the towns of Henbude and Strohteich. But in the town of Dantzig itself there were only 100 cases, of which the greater number were accumulated in houses that still preserved the old system of cesspools.

In like manner the town of Halle had a sad renown for cholera up to the year 1866, but in the year 1873 the disease migrated epidemically from Magdeburg to the suburbs of Halle, and yet the town itself remained unaffected.

These facts, in connection with others, are so remarkable that we cannot avoid asking, What have Dantzig and Halle done since 1866 that, contrary to all experience, they escaped so well in 1873? Dantzig has since that time, aroused by its mayor, Von Wrister, established a regular and rapid discharge for all floating impurities from the houses, and has brought the requisite supplies of pure water from without. Halle also has had its attention aroused by Delbrück's researches, has had its soil carefully examined, and done much towards its purification, and especially has given up its old water service, which was drawn from the Saale at a point above which the sewers of the town opened into the river, and from whence the diluted sewage returned to the houses, and was spread abroad upon the soil of the town. The necessary quantity of pure water was now, therefore, obtained from without.

At all events we see that in Dantzig, as in Halle, some essential link of the chain of the local causes of cholera must have been broken between 1866 and 1873, and however carefully the circumstances may be considered, no other change can be found than the increased purification of the soil. In point of practice, therefore, we may proceed quietly, and look down upon the controversy between the contagionists and localists, allowing them to settle between them in what way draining and supply of water act. That they have an action, neither of the contending parties contest, but all perceive that if the soil of our dwelling-houses be pure it is unnecessary to flee from it during the invasion of cholera.

From this standpoint we may exert a favourable action upon the public health, not alone in small districts, but over the whole of Europe. By a thorough purification of towns in the East the most essential part of the power of the cholera to spread wider and wider, away from its home, until it reaches us, may be abstracted, for it always requires stations and halting-places in the country at certain distances from India, in order, as in its home, to regenerate and acquire new force before it can extend further by intercourse. It is shown by historic evidence that the cholera cannot be introduced through a sea voyage beyond certain distances, or more correctly stated, beyond a certain duration of the journey. England, for instance, has never suffered from cholera in consequence of its uninterrupted and immense marine commerce with India round the Cape of Good Hope, and the Cape itself has never suffered from an epidemic, nor has Australia. England has, in all instances, only been attacked by an epidemic when the cholera has wandered to the European continent through Asia by land, or by short voyages across the Mediterranean. Ships certainly receive on board here and there, in addition to persons who are sick or infected with cholera, also cholera material from cholera places in some mode not hitherto known, which both occasions further infection in the ship, and can be carried, still capable of generation, into other places from the ship. This material, however, if the ship touch no land, constantly dies in the course of three or four weeks; only a few cases are known in which it has been present on board ship for more than a month. The same thing has been observed in caravans, the journey of which through a desert has lasted more than three weeks. The wide waste of waters and wide deserts of sand do not appear to possess those conditions that are requisite for the reproduction of the infectious material of cholera, and which have been developed for thousands of years in the delta of the Ganges and other parts of India. To the soil only do they belong, and only in the soil of those districts in India which are endemically affected by cholera are all the conditions present for the permanent preservation of the poison, and for its periodical rise into an epidemic; the latter occurring more frequently than in other countries, because in India certain meteorological influences which are necessary concomitants of the outbreak occur more regularly and in a more marked form. These various local and periodical conditions are rarely coincident elsewhere. The cholera, as experience shows, is much more frequently brought from India to Bender-Buschir by the Persian Gulf, and to Suez by the Red Sea, than it develops into an epidemic in these places, and it is only when it does so develop that it can be reproduced and spread from these land stations to other regions.

As the cholera wanders from India into Europe only with the aid of certain stations, we may hope that infinitely more may be accomplished in opposing the spread of cholera by cleansing the soil of these stations than has hitherto been done, or than we may in future hope to effect by all the quarantines, and cordons, and inspections in the world; but in order to do something in this direction it is unnecessary to wait till the cholera breaks out, since nothing can be rapidly accomplished, as experience has now sufficiently taught—no, everything must be long previously prepared and carried out. As there are trade and railway organisations, so there must be a sanatory organisation. The improvement of the sanitation of the centres of intercourse in the East is the most important problem for the prophylaxis of cholera, and it is at present the only means which is alike justified by experience and reason. Everything else that has hitherto been done is simply a series of useless, transient, involuntary convulsions, spasmodic movements, into which we fall as often as the evil surprises us, but which, however energetically they may be carried out, do not in the smallest degree alter the conditions that may be present.

FOUL WATER AND ITS INFLUENCE ON THE PUBLIC HEALTH.

It is natural that the report of the Rivers' Pollution Commission on the Domestic Water Supply of Great Britain should have greatly perturbed many minds, especially in the Metropolis. The revolting description of the filth—human, brute, and miscellaneous—which is poured into the streams from which the London Water Companies obtain their supply above their intakes, would revolt the most insensitive mind. But in the various movements which have taken place since the publication of the report, in protestation against this disgusting state of things, it is instructive to observe how an instinctive disgust, rather than a well-reasoned belief, has guided the protestors. The observations of the Royal Commissioners on the absence of evidence of mischief to the public health arising from water fouled, as they describe Thames and Lea water to be, under ordinary circumstances, hardly seems to have arrested attention. Yet when the question is brought to a practical issue, this opinion will necessarily affect very considerably the result. If from the combined results of filtration, dilution, and the possible indifference of the human economy to filth in certain forms, the water is harmless, why should we, to meet a mere sentiment of disgust, still further augment the already overgrown metropolitan rates?

For our own part we believe that the instinctive disgust of the protestors is right, both sentimentally and physiologically. We do not believe that there is so complete an absence of evidence as to mischief from water polluted with ordinary sewage matters, as the Royal Commissioners seem to think. The evidence of this mischief is not perhaps so obvious as that arising from the dissemination of the infections of cholera and of typhoid fever through water, but it may perhaps be inferred that this is the case rather from the mischief of its not having been yet discriminated, than from its own existence. This is a question which is receiving the careful attention of medical officers of health in rural districts; and, so far as our knowledge goes, they are of accord in attributing much of the sickness which affects children to the use of filthy water. The question is one which, the Rivers' Pollution Commission having dealt with it perfunctorily, should be relegated, in the present state of public opinion, to the Local Government Board. It is one which the Medical Department of that Board could well deal with, and which it is eminently desirable that it should deal with. The more closely the modes of operation through which filth becomes mischievous are studied, the less likely does it seem that the filth distributed in waters like those of the Thames and the Lea can be consumed without some ulterior results. Dr. Burdon-Sanderson's late researches on the septic properties of certain putrefying matters point to grave possibilities of mischief in foul water which cannot be ignored. While the protestors, who are moving the Government as to the water-supply of London, are careful to describe its disgusting character, they would do well to ask for further information as to the opinion of the Rivers' Pollution Commission on the presumed harmlessness of ordinary sewage when diffused in drinking water, and to press for a special investigation of the question.

[On account of press of matter, the REVIEWS, BIBLIOGRAPHY, and NOTES, are unavoidably postponed.—ED. PRACT.]

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ON THE PROCESS OF FEVER.

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(*Continued from* p. 358.)

PART III.—PYREXIA.

Section I.—The Norma of Temperature.

THE temperature of the healthy human body has been recently determined by Jürgensen[1] on a larger basis of accurate measurements than before existed. The mean temperature of day and night of a healthy person, as measured in the rectum, is 37°·2 C. (98·9 F.) If the whole period of 24 hours is divided into two, of which one, commencing at the moment that the bodily temperature reaches its morning minimum, ends with the attainment of the evening maximum, the other corresponding to the interval, it is found that in the diurnal period, viz., between 7 or 8 A.M. and 9 P.M., the mean temperature is 37°·34, while in the nocturnal period, of which the duration is shorter in the proportion 100 to 136, the mean temperature is 36°·94 (98·5 F.)

In relation to our present inquiry, Jürgensen's researches have led to a most remarkable result, which was entirely unexpected, viz., that the *mean* temperature of the human body is remarkably independent of the conditions which temporarily affect the production of heat, even when their influence is most powerful. Thus, by observations on a vigorous and healthy person who voluntarily submitted to inanition for 63 hours, it was found not only that the mean temperature of the two days was exactly the same as in ordinary conditions of nutrition, but that the diurnal temperature curve was not modified in its character.[1] Again, it was ascertained by the most accurate observations on the same patient, who submitted to a succession of cold baths, each lasting 25 minutes, at temperatures varying from 9° to 11° C. (50° F.) that, notwithstanding the rapid abstraction of heat, which gave rise to a shivering lasting for several hours, the diminution of bodily temperature which occurred during the bath was followed, after an interval of four or five hours, by an elevation which precisely compensated it, so that if the average was taken of observations extending over a sufficiently long period, the mean was the same as under normal conditions.

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[1] Jürgensen, *Die Körperwärme des gesunden Menschen.* Leipzig, 1873, p. 9.

An exactly similar result was come to as regards muscular work, showing that as in the former case the depression, so here the elevation of temperature by active exercise is, in the long run, completely made up for.

When it is remembered that all these conditions—inanition, abstraction of heat by the cold bath, and muscular work—are known to produce very considerable effects on the exchange of material in the body and consequently on the thermogenesis, they afford the strongest possible evidence that increased or diminished temperature has no necessary connexion with increased or diminished production of heat.

The extension of Jürgensen's observations to fever, led him to the remarkable discovery that in continued fever, and particularly in typhoid, the diurnal variation of bodily temperature closely resembles that of health; so that if two curves representing these variations are inscribed one above the other over the same abscissa, they are seen to run parallel to each other with a constancy which is very remarkable. In other words, the only material difference between the two conditions is that in fever, the normal is 3° higher (40°·1 (104°·2 F.) to 40°·3 instead of 37°·). Whatever be the explanation of this, the fact comes out so clearly as the result of observation, that it cannot be disputed. It affords evidence that the laws which regulate the temperature of our body, and to which even the chemical activity of our protoplasm is subject, are not upset in the febrile state, and gives us ground for anticipating that, whenever those laws shall have been fully investigated, they will be found to be the expressions of the most fundamental and specific endowments of our being.

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[1] Jürgensen, l. c. p. 21.

That the shifting of the abscissa of the temperature curve three degrees upwards, does not, as Liebermeister thinks, signify that in fever the organism has adopted a new norma of temperature, is proved by the observations of the most accurate clinicists as to the influence of antipyretic remedies, and particularly of cold and quinine. We have already seen that in health "the law of temperature" is strong enough to withstand even the most rapid abstraction of heat from the surface; but this is not so in fever. Clinical observation has established the fact (whatever may be its therapeutical value) that in typhoid and other fevers the temperature can be brought down and kept down nearly to the normal by the systematic use of the cold bath, or of the cold affusion. Hence we are justified in concluding that in fever, conformity with the law of temperature is not abolished, but merely weakened; that just as in health a *nisus* constantly exists towards excess, which is restrained by the controlling influence of that "*schützendes Etwas*," the existence of which we unconsciously refer to when we call it the law of temperature; so in fever that influence still exists and needs only to be strengthened or aided; strengthened when we administer large doses of quinine, aided when by the cold bath we abstract heat from the surface. It is on this principle only that we can understand how it is that those anti-pyretic agents, of which the action is so certain in fever, have no permanent effect on temperature in health.

Section II.—Relation between Pyrexia and the other Functional Disorders of the Febrile State.

Whenever two actions or groups of phenomena are associated so as to constitute a process, the theory or explanation of that process consists in determining what is the nature of the association. And if we designate the two associated actions or phenomena respectively *a* and *b*, then the possibilities to be considered are that *a* may be the consequence of *b*, that *b* may be the consequence of *a*, or that both may stand side by side as collateral products of some other antecedent. So in the case of fever with its two chief phenomena pyrexia and textural disintegration, the theory of the process is involved in the question whether the former or the latter is primary, or whether both are not the results of an action originating independently of either.

I have referred to these questions not with a view to their immediate discussion, but for the purpose of indicating what form should be given to our investigation of the subject, in order that eventually we may find ourselves in a position to answer them.

The temperature of the body being dependent on the balance of the production and discharge of heat, of which the former is a function of living protoplasm, the latter a function of the organs of circulation, respiration, and secretion, the question whether pyrexia or textural disintegration is primary, comes to assume the form in which it has been frequently discussed by pathological authors, viz., whether it is production or discharge which is primarily disordered.

Want of information as to the normal relation between the former of these processes and the bodily temperature, renders it impossible to answer the question in either of its forms, in other words, to frame a theory of fever. All that can be attempted is to study the mutual reactions on each other of the two sets of functional disorders, in order that when these relations have been ascertained by observation, we may be furnished with such anticipations as to their nature, as may guide us in clinical and experimental researches. With this view I shall occupy the remainder of this section, by giving an account of the agencies which exist in the animal body for regulating the discharge of heat, stating with respect to each of them what is known as to its nature; for inasmuch as we as yet *do not* know by observation or experiment of any mechanism, any endowment of living protoplasm by which it can regulate or limit its own chemical, *i.e.*, thermogenetic processes, and *do* know how the escape of heat from the surface is limited, it is clearly necessary to satisfy ourselves by the most careful observation that these agencies are inadequate, before we fall back, as I anticipate we shall eventually do, on a process having its seat in protoplasm, and seek for the explanation of the regulation of temperature in the establishment of a direct relation between it and the functional activity of living tissue.

a.—Discharge of Heat by Sweating from the Skin.

This subject has already been discussed in connection with Leyden's researches on febrile thermogenesis. No precise measurements exist of the quantity of moisture (aqueous vapour) discharged by the human skin in health, and consequently no estimate can be formed of the heat normally lost in this way. According to Helmholtz's calorimetrical investigations the total loss of heat by the skin amounts to about 1,800 to 2,000 kil.-units daily;[1] Leyden's experiments have shown that this loss would be doubled in the moderate sweating of the febrile crisis, if the whole of the heat contained in the perspired water were abstracted from the organism. As has been already pointed out, this is never likely to be the case, for however well the body of a patient may be ventilated, the ventilation is never sufficient under ordinary conditions to carry away the secreted water as vapour as fast as it is discharged by the sweat glands. If it were sufficient, the abstraction of heat from the body would be extremely rapid. We have no data for estimating it in fever, because we have no measurements of the quantity of water actually discharged. In the Turkish bath the loss of water often amounts to 500 to 600 grammes per hour. If this weight of water were entirely converted into vapour it would imply a loss of heat amounting to some 300 to 360 k.-units, which (supposing the specific heat of the body to be nearly equal to that of water) would cool it 6° or 7°. In fever the quantity of liquid sweated can never or very rarely be comparable to that discharged in the Turkish bath, but even here, if it were completely converted into vapour, the heat loss would be very rapid.

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[1] It may be well to notice that the word "calorie" used by Prof. Binz in his admirable Paper on Febrifuges in the last number of this Journal means the same thing as kilogramme-unit.

b.—Discharge of Heat by Respiration.

The normal loss by respiration may be estimated as about 320 to 330 k.-units daily, made up of 80 to 90 k.-units, spent in warming some 10,000 litres of air by about 25° C., and 240 k.-units used in evaporating 412 grammes of water. This loss may obviously be increased by any cause which increases the activity of respiration.

Thus in fever, when in general the quantity of air breathed has been shown by Leyden's researches to be increased by more than 50 per cent., it is probable that other things being equal, both the evaporation loss, and that incurred in warming the air respired, will be correspondingly increased; but, considering that the total respiration loss, according to Helmholtz, amounts to about 20 per cent., the pulmonary cooling is without doubt of little importance as compared with that which takes place by the surface of the skin.

In the dog the influence of respiration on temperature has been shown by Dr. Riegel to be much more considerable, and has an unquestionable importance as a regulating agency. In that animal any cause which elevates the bodily temperature, whether this is effected by clothing the body in non-conducting material, or placing it in a warm environment, excites respiration in a very remarkable manner, so that the animal not only breathes more frequently, but inhales a larger quantity of air at each breath. This effect, which must be attributed to the influence of the increased temperature of the blood on the intracranial nervous centres, was first described by Ackermann under the term heat-dyspnœa. It is not, however, so much with reference to the mode in which it is produced, or to its teleological significance, that it is now referred to. Riegel[1] has shown that in the heat economy of the dog, the function of respiration is the most important means which exists for the maintenance of constancy of temperature. When a dog is observed in a moderately overheated room, say at 25° R. (88° F.), a temperature which to the human organism is not uncomfortably high, it soon becomes distressed and breathes with extraordinary rapidity. If under these circumstances its temperature is observed continuously, it is found scarcely to exceed the normal; but if, while still in the warm room, it is placed fully under the influence of chloroform (a drug which under ordinary circumstances invariably decreases the temperature of the dog), its temperature rises at once to that of pyrexia. In this experiment the reason why the temperature rises, is evidently that the narcotic renders the nervous system incapable of reacting against the stimulus to which it is exposed. Consequently the rise of the temperature of the blood is no longer the signal for that more active breathing by which, if the animal were not narcotized, the normal state of things would be restored.

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[1] Riegel, *Ueber den Einfluss des central Nervensystems auf die thierische Wärme.* Pflüger's *Archiv*, vol. v. p. 651.

c.—Influence of Changes in the Circulation on the Discharge of Heat.

The explanation which is commonly given of the nature and cause of febrile pyrexia is founded on the supposition that in health the constancy of the bodily temperature is dependent on the power which the organism possesses of regulating the circulation, that is to say, that the faculty which man in common with other homoiothermic animals enjoys of maintaining equability of temperature, depends on the existence in his nervous system of apparatus by which the relative quantity of blood contained in those parts of the vascular system which are exposed to the influence of external media (particularly in the vessels of the lungs and skin), can be increased or diminished according to the requirements of the organism. In constructing a theory of fever on this basis, it is assumed that the disorder of the circulation is the first link in the chain of phenomena of which the febrile process consists, and consequently, that fever has its origin in disorder of the nervous system. For inasmuch as it is known experimentally that the distribution of the blood is entirely regulated by "vasomotor centres," it is clear that no departure from the normal condition of the circulation can happen, otherwise than by virtue of an antecedent disturbance of the functions of those centres.

Until a very recent period the knowledge which existed as to the influence exercised by variations of the velocity of the blood-stream and of the distribution of the blood on the bodily temperature was chiefly theoretical, consisting rather of deductions from what was known as to the mechanism of the circulation than of results derived directly from observation. During the last few years a more or less complete experimental basis has been furnished by the physiological researches of Heidenhain, as well as by the pathological inquiries of Riegel and Naunyn,[1] so that although there are still innumerable questions which must be left open, there are some points on which it is possible to speak with confidence.

One of the most satisfactory and (if I may be permitted the expression) one of the most elegant proofs that by increasing the quantity of blood in circulation in an exposed part, the temperature of the whole body can be diminished, is afforded when the vessels of an exposed organ are paralysed by the severance of its vasomotor nerves, so that while the capacity of its relaxed blood-channels is largely increased, the force of the circulation remains unimpaired. Thus, when in the well-known experiment which twenty years ago led to the discovery of the vasomotor nervous system, the vascular nerves of the rabbit's ear are divided, the increased warmth thereby produced in the ear is associated with a diminution of the temperature of the whole body, which though small in amount is perfectly measurable. It is small because the quantity of blood which is in circulation at any moment in the paralysed organ is a mere fraction of the whole mass of the contents of the vascular system.

When the vessels of the whole or of the greater part of the body are thrown into a state of relaxation by severance of the spinal cord, the resulting abnormalities of bodily temperature are much more considerable, but their nature and degree depend rather on the secondary effect which the total disappearance of arterial tonus exercises on the circulation, than on the altered distribution of the blood-current. For when all the arteries are relaxed, the velocity of the circulation, *i.e.*, the quantity of blood discharged by the heart in a given time, is necessarily diminished, the reason of this being that, while the mass of blood contained in the whole circulatory apparatus (heart and blood vessels) remains unchanged, the capacity of the vascular system is augmented, and consequently that of the heart diminished. The heart, therefore, containing a smaller quantity of blood at the commencement of each contraction, discharges less, *i.e.*, works with less effect.

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[1] Dubczcanski und Naunyn, *Beiträge zur Lehre von der fieberhaften Temperaturerhöhung. Archiv für Pathologie,* vol. [illegible] p. 181.

Although it might have been anticipated from previously existing knowledge as to the mechanical conditions of the circulation, that this would be the case, *i.e.*, that abolition of arterial tonus would be disadvantageous to the circulation, the fact that it is so was not only not recognized, but, by many physiological writers distinctly denied. The mechanical physiologists of the French school, whose doctrines have been popularized in this country by the works of M. Marey,[1] teach without any reservation that the effect of general relaxation of the blood vessels is to increase the force of the pulse, and to augment the frequency of the contractions of the heart, and found upon this notion their theory of fever.

d.—Comparison of the State of the Circulation in Fever with its State after Injury of the Spinal Cord.

If in man or in an animal, severance of the cord takes place below the origin of the phrenic nerves, the respiratory movements, although altered in their character by the paralysis of the muscles of the trunk, continue, and consequently life is preserved. The phenomena which present themselves in these cases, from their extreme interest in connection with the heat economy of the living body, have of late years concentrated upon themselves the attention of pathologists. Those which are of most importance in relation to our present inquiry are the following:—

1. For the reason already stated, section of the cord at the level of the seventh vertebra, or accidental destruction of the organ

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[1] See Marey's *Physiol. de la Circulation*, chap. xviii. p. 3501.

in man, produces as its primary effect an enormous diminution of the pressure on the arteries, in consequence of which the circulation diminishes. The influence exercised by this diminution on the temperature of the body varies both in degree and direction, according to the animal observed, and the nature of its environment. In man, as is well known from the historical case recorded by Sir Benjamin Brodie, as well as from others published since, it often happens that the bodily heat rises above the normal, sometimes above that of fever. In small animals, such as rabbits, the temperature falls rapidly, notwithstanding that they are so well clothed in fur, and that the observation is made in summer weather. In animals of intermediate size, such as dogs, the result varies according to the outside temperature, and the protection which is afforded against surface cooling, either by the natural covering or by artificial clothing. Thus, if the creature is invested in the envelope of non-conducting material, and kept in an ordinary room, the temperature of the blood inevitably rises to a height which often exceeds that of fever. Without clothing, especially if it be of relatively small size, its temperature rapidly sinks until it dies in collapse.

From these facts it is obvious that the most striking difference between the heat economy of the injured and that of the normal animal consists in this, that in the latter the bodily temperature is independent of external conditions, whereas in the former it is affected even by very slight changes in the environment. We have already seen to what an almost incredible extent this is true of man. Similarly, it is found that a healthy dog accustomed to the temperature of summer, may be exposed to that of a winter's day without the slightest variation. After the cord is divided, the same animal must be clothed, even when in a warm room, else it cools too much; while if the room is only a very little too warm, it passes into a state of intense pyrexia. To prevent either result, depression on the one hand or collapse on the other, the surface loss has to be accurately adjusted to the thermogenesis by artificial means; for the animal has lost its power of making any adjustment for itself.

As regards the heat economy of animals after section of the cord at the level of the sixth or seventh vertebra, there has been much dispute. Most observers have been content to assume, as a sufficient explanation in the case of depression of temperature, that the result is attributable to the paralysed condition of the cutaneous vessels and to the consequent increased discharge of heat at the surface. As regards the other cases, it has been generally taken for granted that there is a close correspondence between the condition induced by the injury and that of fever, and accordingly that there is, along with the augmented disengagement of heat from the surface, an associated increased activity of those powers by which heat is produced; so that the former is more than compensated by the latter. In order to bring this view into harmony and connection with Virchow's well-known doctrine of an inhibitory centre situated somewhere in the intra-cranial nervous system, all that is necessary is to suppose that just as in clinical fever the influence of this centre is directly paralysed by the morbific agent, so after section of the cord its influence on the tissues is annulled by the severance of the channels by which it is normally exercised.

This theory can now no longer be accepted in its application to the phenomena observed after section of the cord; for it has been shown experimentally by Dr. Murri that even when the temperature rises the thermogenetic function may undergo great diminution, and that in the contrary case there is no evidence of increased liberation of heat. It is a fundamental and well-known fact in relation to the distribution of temperature in the animal body, that the greater the vigour of the circulation, the less is the difference of temperature between the central and superficial parts; so that by measuring simultaneously the temperature of the skin and that of the rectum, we are enabled to judge very accurately of the velocity of the blood-stream. Why this is so, is easy to understand. When the arteries are distended, and the blood current is rapid, a larger quantity of blood is brought to the surface from the warmer central parts of the body than in the contrary case. The mass of blood therefore being greater, the cooling it sustains by exposure to the cooler environment of the body is proportionately less, so that it returns by the veins towards the centre at a temperature which does not differ largely from that which it possessed in its outward course. Consequently if we find the temperature of the surface much less than that of the central parts, we are quite certain that the circulation through the surface vessels is feeble, and that little heat is being given off. Now Murri's experiments appear to show that even when after section of the cord, the internal temperature was normal or excessive, that of the skin was unnaturally low, so that the condition of the circulation was such as to be least favourable to the discharge of heat from the surface.[1]

The only fact therefore which in the present state of our knowledge can be accepted as characteristic of the thermal state of an animal after section of the cord, is that of the abnormal facility with which the bodily temperature yields to the influence of outside conditions—the fact that the temperature equilibrium, to use Naunyn's expression, becomes labile. This being the case, it is obvious that the analogy between this condition and that of fever is not sufficiently close to justify us in following the example of those pathologists who have founded on it a theory of fever. As, however, it may be of use in enabling us to answer some inevitable questions, and particularly may facilitate the understanding of the relation between thermogenesis and temperature in fever, time will not be wasted in instituting a comparison between the two kinds of pyrexia.

In the various fevers which form the subject of clinical observation, no very accurate information exists as to the state of the circulation. The comparison of the central with the surface temperature, which in the study of the effects of section of the spinal cord, affords us such valuable information, would also be of value if it were made in the early stage of fever; but as yet clinicists have not recognised its importance, so that the necessary basis of observation is wanting. In the later stages of fever the introduction of another element, that of sweating, a heat-discharging process which is in great measure independent of the circulation, deprives the observation and measurement of the surface temperature of much of its value as a criterion.

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[1] Dr. Murri's experiments are recorded in his following papers:—(1.) *Del potere regolatore della temperatura animale.* Florence, 1873, and (2.) *Sulla teoria della febbre.* Fermo, 1874. His result, although of great importance, can hardly as yet be considered conclusive.

In the first part of this paper it has been shown that as regards the ordinary sources of heat, viz., those dependent on the ingestion of food, the diminution of thermogenesis is so considerable that the total quantity of heat produced in the body in a given time, even when augmented by the quantity arising from the abnormal disintegration of tissue, may still fall short of the average of health. This being the case, it is natural to ask, how it is that we do not meet with instances of fever, in which (as after section of the cord in small animals and under external conditions favourable to cooling), the temperature remains stationary or sinks instead of rising. The answer to this question is, that although, as might be expected, we have perhaps no counterparts in human pathology to those relatively common cases, in which, after accidental injury of the cord, the patient becomes cold instead of hot, such instances are familiar to an experimental pathologist. We have seen that in the dog febrile pyrexia may be induced with certainty by the introduction into the circulation of a known dose of pyrogenic substance. If a dose of the same substance, proportionate in quantity to the smaller weight of the animal, be injected into the circulation of a rabbit or guinea-pig, the same succession of disorders occurs with this difference, that the temperature falls *below* the normal instead of rising above it, the amount of depression varying according to the temperature of the environment. Here, therefore, just as after section of the cord, we have evidence that the thermogenesis *may fall below the normal limit of adequacy:* the cause, which in a larger animal would produce fever with all its signs, here produces only an intense general disorder with diminished heat of body and diminished thermic discharge.

e.—Concluding Inferences as to the Seat of Origin of Febrile Pyrexia.

A satisfactory explanation of the nature of fever and of its relation to the febrile process is not at present possible, because we are not as yet possessed of the necessary physiological knowledge. We have elsewhere stated that two possibilities are open to us. One is, that fever originates in disorder of the nervous centres, that by means of the influence of the nervous system on the systemic functions, the liberation of heat at the surface of the body is controlled or restrained, so that "by retention" the temperature rises, and finally that the increased temperature so produced acts on the living substance of the body, so as to disorder its nutrition. The other alternative is that fever originates in the living tissues, that it is from first to last a disorder of protoplasm, and that all the systematic disturbances are secondary.

In both hypotheses it is tacitly assumed that fever is the product of a material fever-producing cause contained in the blood or tissue juice, the morbific action of which on the organism is antecedent to all functional disturbances whatever. At bottom we are all humoralists, and believe in infection. It is not until we have to say where and how the infection acts that questions arise.

The facts and considerations we have had before us are, I think, sufficient to justify the definitive rejection of the first hypothesis in all its forms; for, on the one hand, we have seen, that no disorder of the systemic functions or of the nervous centres which preside over them is capable of inducing a state which can be identified with febrile pyrexia; and, on the other, that it is possible for such a state to originate and persist in the organism after the influence of the central nervous system has been withdrawn from the tissues by the severance of the spinal cord.

We are therefore at liberty to adopt the tissue-origin of fever as the basis on which we hope *eventually* to construct an explanation of the process. But if we attempt to do so *now*, we shall at once find ourselves in face of an unsolved physiological problem, that of the normal relation between temperature and thermogenesis, for the elucidation of which it is necessary to investigate much more completely than has yet been found possible, the influence of temperature variations on those chemical processes in living tissue, with which thermogenesis is necessarily associated. The little that has been already accomplished in this direction is sufficient to show that the living substance of our bodies is, if I may so express myself, delicately sensitive to variations in the temperature of its environment, so that very slight deviations from the normal may produce effects of surprising magnitude.

NOTE.—This paper was published in the Appendix to the Report of the Medical Officer of the Privy Council on Scientific Investigations, New Series, No. vi. It is reprinted with the permission of the Medical Officer.

The attention of the author has been kindly drawn to an important error in the first part of this Paper, on p. 280, four lines from the end. The difference between 32·3 grammes (the supposed total discharge of CO_2 per hour) and the weight of CO_2 corresponding to the amount of carbon contained in the hourly urea discharge, viz. 8·5 grammes, is 23·8, not 28 as printed.

ON FEBRIFUGES.

BY C. BINZ, M.D.,[1]

Professor at the University of Bonn.

(*Continued from* p. 366.)

QUININE.

WE have seen that to alcohol belongs, not only the indisputable property of dilating the vessels of the skin, and thus promoting the irradiation of warmth in fever, but, in all probability, that of checking the activity of the cells as well, and in this way directly diminishing the production of heat in the tissues. In quinine and salicylic acid we have antipyretics, which probably exhibit only the latter action.

Nowhere was the neurotic theory of fever more clearly reflected than in the manner in which the action of quinine was formerly understood. Quinine was a tonic, nothing more nor less; that is to say, it gave the nervous system strength to resist the fever, and for that reason the latter disappeared, sometimes rapidly, sometimes slowly.

Facts, however, all speak against this one-sided conception of the matter. Ague, for instance, is to be regarded as the fever in which quinine gives the most brilliant results. It arises from the absorption into the system of a specific poison which develops itself from decaying vegetation. If this poison circulates too long within the body, decomposition of the blood is the result. Among the phenomena presented by the disease, the "neurotic" febrile symptoms may be entirely wanting, while the appearances depending upon degenerative changes in the blood and in the tissues, never fail to show themselves, though at the beginning they are often only slight.

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[1] In the two preceding numbers, LOND. has been erroneously placed after Professor Binz's name, on account of a misreading of the word Bonn.

Now, the notion that the nerves should be able to resist or to destroy any putrid matter by the help of quinine, is quite unintelligible according to our present knowledge of such things. First of all, we see that ague is cured by quinine most efficaciously in the time of apyrexia, when the infectious poison is latent, when every part of the nervous system is free, when neither heart nor respiration shows the slightest disturbance. Besides that, experimental researches have failed to demonstrate the presence of any such influence of quinine upon the nervous system as it has been supposed to possess. The influence of quinine over the febrile process in general may take place without the brain being at all acted upon. Thus, section of the spinal cord at the level of the first cervical vertebra does not prevent this antipyretic influence from being manifested, although, in that condition, not only is the brain separated from the cord, but the chief vaso-motor nerves are paralysed as well.

The diminution of reflex excitability in the nervous system, so much spoken of as having been observed in animals treated with quinine, has been proved by recent and strict investigations made in my laboratory to be purely imaginary.[1]

In former times, before it was known how fever can be produced artificially, one used to remark the improved condition of the fever patient after a good dose of quinine, and to attribute it to the "neurotonic" action of the drug. To-day, we know that this improvement depends upon a decrease in the oxidation and disintegration of the elements of the body, as especially shown in the much lower percentage of urea contained in the urine discharged after a dose of quinine. And as the absorption of certain ferments is evidently the cause of many fevers, it was worth while to investigate whether this agent has any specific power over them.

After the isolation of the various alkaloids from their respective drugs (dating from the year 1816), when it was found by experiments upon animals that these substances, even in very small quantities, all exerted an influence either upon nerve-centres or upon nerve-trunks, this property came to be regarded as one belonging to alkaloids in general, and thus quinine, discovered in 1822, easily got the same reputation. No doubt it possesses a striking family resemblance to these nervine alkaloids. It differs from them, however, in one important point and in a very remarkable degree.

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[1] Heubach, *Archives of Experimental Pathology and Pharmacology*, vol v. pp. 1-38. (Leipzig.)

All the other alkaloids in common use act in very small doses; caffein, which possesses this characteristic least of all, produces its characteristic effects even when given in a dose of a few grains. Quinine, on the contrary, given in minute quantities, like morphia, strychnia, atropia, is absolutely indifferent as regards any effect produced. The hundredfold of such doses is the quantity in which it must be taken in order to bring about the result it is desired to obtain.

This striking difference ought to have given a hint as to where to direct the researches in connection with quinine. Pringle, in 1750, made a beginning with cinchona bark only, which, if followed up, would have given results of great importance, but the nerve theories appeared, and soon stifled these valuable experiments.

Quinine is a powerful antiseptic and antizymotic, possessing decided advantages over all other agents of this class. It can be given in large doses,—it remains in the circulation for many hours,—it does not enter into combinations or undergo decomposition within the organism so as to become a thing chemically indifferent with respect to the ferments upon which it is desired to have it act. When given once a day in a dose that is not of itself too small, it circulates through the tissues, keeping them constantly under its influence.

An agent which acts antiseptically outside of the body cannot fail, under such circumstances, to exhibit the same action within it. Ague derives its origin from septic processes occurring in the blood and tissues, and if the disease progress unhindered, it will in many cases finally destroy life with all the symptoms denoting decomposition of both. If brought into contact, without the system, with the exciting cause of ague—which, as we know, is a ferment arising from decaying vegetation—quinine will paralyse the further chemical development of the ferment itself, just as it paralyses the activity of processes set up within the body by this subtile agent, unknown as to its precise nature, but well understood in its effects.

Quinine has no permanent influence upon the temperature of the body in health, although it often produces a distinct change, as well as causing a decrease in the amount of urea excreted. We need not recur to any nerve theory to explain this. Quinine has a very strong affinity for many septic ferments, but a very slight one for the ferments of the healthy organism, as can easily be proved by experiment. Therefore, wherever it encounters one of the former in the course of its circulation, it manifests this affinity, paralysing the activity of the pyrogenic substance, so that, as a result, a change of temperature is clearly shown by the thermometer; but wherever these septic ferments are absent, this result will, of course, be much less perceptible.

If one severs in a living dog all the nerves leading to the spleen, quinine nevertheless acts on that organ, causing it to shrink. The spleen is the chief source of the formation of uric acid. Quinine lessens the quantity of this acid produced by a healthy man very strikingly. As the afferent nerves of the spleen are not necessary in order that this action may take place, we are induced to accept as an explanation of the same, the probable occurrence of a chemical process in this instance also. This conclusion is especially justifiable, as the spleen contains an immense number of white cells, for which quinine is a highly efficacious poison. They are the seat of energetic oxidation, and this oxidation is lowered by quinine.

In febrile processes the spleen becomes swollen in consequence of the irritation it receives from the pyretic substances that circulate in the blood. Now if the activity of these substances be diminished by any means, it follows that the effect they have contributed to produce will also experience a diminution, and nature once more exerting her supremacy, the spleen will shrink to its normal size.

The decline of temperature in fever, so generally induced by quinine, takes place independently of any connection with the performance of the heart's functions. It very often happens, in fact, that a change in the action of the heart only *follows* such a decline. The same is true with regard to the function of respiration.

Of all that has hitherto been said, especially in France, about the antipyretic action of quinine through the medium of the vessels, nothing has as yet been proved, and the evidence against such a theory is abundant.[1]

In the March number of this periodical I read a very interesting article on the treatment of sunstroke by the hypodermic administration of quinine. Dr. Hall thinks that quinine acts here as a stimulant to the vaso-motor and the inhibitory nerves. Dilated capillaries in sunstroke may be produced by stimulation of the inhibitory centres and depressor nerve of the heart, or by exhaustion of the vaso-motor centres. Quinine may reduce the temperature of the skin by exalting the action of the inhibitory apparatus, and allowing the vaso-motor centres to produce contraction of the blood-vessels; or may act as a direct stimulant on the vaso-motor centres themselves.

The author of that paper mentioned my name in connection with this view. He must have misunderstood me, as after all my experiments,[2] I am more than ever convinced that we have no right as yet to such opinions.

The essential cause of sunstroke is overheating of blood and tissues, and therefore decomposition of the nutritive material. The blood acts as poison on the heart and the nervous system. Should the treatment recommended by Dr. Hall prove applicable to all cases, we must assume that here also quinine acts as a direct antizymotic, as it does in malaria fever.

Numerous are the instances in which it has been shown that quinine acts directly on protoplasm, and without any agency of the nerves being involved. Take, for instance, two similar specimens of any protoplasm which is known to undergo rapid oxidation when exposed to the air, and add a very small quantity of quinine in a soluble form to one of them. The difference of effect produced upon the two by the oxygen of the air will soon be observed, the quinine checking the combination completely where it has been applied. One of the most striking examples of this anti-oxidizing power is shown in the following experiment.

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[1] It would exceed the space at my disposal were I to enter into experimental details. I have summed them up, with references to the different literary sources in my pamphlet: *Das Chinin, nach den neuren vharmakologischen Arbeiten.* Berlin, 1875.

[2] *Archiv f. exper. Pathol. u. Pharmakologie.* December 1875.

Place a piece of a fresh sea fish in sea-water, and after a certain time, depending upon the warmth of the season, before any putrefaction takes place, the whole becomes brilliantly phosphorescent. There can be no doubt of the fact that this phosphorescence is derived from the energetic oxidation of the numerous infusoria contained in the fluid.[1] This luminous water is next put into different test-tubes and acted upon successively by various antiseptic agents, beginning with a very small quantity of neutral quinine (1 to 5,000 or more), then by a very weak solution of carbolic acid, and by the different neutral salts belonging to the class of antiseptics, all, of course, dissolved in distilled water, and experimented with in a perfectly dark room. Quinine is then found to surpass them all in the energy with which it causes the phosphorescence to disappear.

If it were claimed that quinine is incapable of acting chemically upon living cells in such a state of dilution as it circulates in the body, the above experiment, among others, would prove the contrary. Only one must not expect to obtain a quite sudden effect, using such minute proportions, nor do we expect anything of the sort in the case of a fever patient to whom we administer such doses. If it be desired to destroy the oxidability of the sea-water infusoria within half-an-hour, one may take quinine in the proportion of 1 to 12,000; if in a few minutes, 1 to 2,000; if in an instant, 1 to 200. Agitation of the previously luminous liquid with air, a proceeding which would otherwise revive the phosphorescence of the protoplasma, then fails to produce this effect.

I should not like to be misunderstood, however, with respect to the purpose for which I have introduced this illustration, for I do not in the least pretend that such animalculæ are in any way involved as the exciting cause of fevers, but have merely taken their protoplasm as *one* type in which quinine exhibits, in a striking manner, its anti-oxidizing power, just as in other experiments it shows an antizymotic one.

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[1] For evidence bearing upon this point, see Pflüger's *Archive für Physiologie*, 1875, xi. 222.

All these questions, of course, as to the precise manner in which quinine produces its antipyretic action, can only be settled when we shall have become better acquainted with the nature of pyrogenic poisons. Here I can do no more than point out the path which, if followed, promises to lead to the desired knowledge on this subject, and at the same time to show how the theory formerly offered in explanation of the same only led us into a "blind alley," so to speak, from which there was no exit but to retrace one's own footsteps.

Knowing at some future time the different agents concerned in the production of each separate form of fever, it will be easy to understand how it is that quinine is so extremely efficacious in ague, while it fails completely in relapsing fever; why it acts with such decided antipyretic power in *Typhus abdominalis*, while it seems to be useless in *Typhus exanthematicus*, and so on.

The well-marked action exerted by quinine in solution, upon many septic ferments on the one hand, and its indifference toward the protoplasm of *Penicillium glaucum*[1] on the other, may serve for the present as an instance illustrating by tangible examples the puzzling inequality of effects produced by this drug upon fever-producing ferments within the human body.

As to the nervous system, I do not pretend to claim that quinine is entirely without influence upon so important a part of the animal mechanism, for sleep is often produced by a dose of quinine in a non-febrile illness, and certain neuralgic affections are known to be completely under its power. Like alcohol and calomel, therefore, quinine may be said to possess a manifold sphere of action. As regards its efficacy in fever, we must once more confess our inability to explain all the facts taught us by therapeutic experience. But, as yet, the only theory which reconciles the results of such observations with the results elicited by experiment in the laboratory, is the humoral one.

I will conclude with some practical remarks as to the application of quinine in fever. It is to be given (1) in large doses from 0·5 to 3·0 grammes (8–48 grains); (2) in a digestible form, that is to say, together with some acid; and (3) during the time when fever tends to decrease.

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[1] Cf. Virchow's *Archiv*, xlvi. 73.

The first has been proved by numerous observations in the sick-room. Such strong doses are not poisonous, but agree on the whole better with young than with old people. My second rule refers to the fact that in fever the stomach does not contain sufficient acid to dissolve the sparingly soluble sulphate. As to the hour of administration, we have to deal here, as a matter of course, with similar conditions as in ague fever. The cause of fever is overcome by quinine most easily when its activity is lowest.

The complaint has often been raised that quinine so easily causes sickness, and it is generally supposed that the vomiting is produced by direct irritation of the stomach. This is not proved to be the case. On the contrary, if the doses of quinine are continued, at the most only a small part of the second or third are ejected. All later ones remain. The stomach absorbs it as hitherto, the nervous system by degrees tolerates the alkaloid, and now only it begins to act upon the chemical processes of the feverish organism. It is very important not to make use of impure quinine. Quinine containing the less efficacious cinchonine is the most deceptive, for this adulteration is somewhat difficult to detect, at least for many practitioners. Much the same must be said of salicine, the bitter principle of willow.

By adding to a solution of *pure* sulphate or hydrochlorate of quinine a little ammonia, one gets a thick precipitate of hydrate of quinine. This is easily soluble in a little ether, whilst the hydrate of cinchonine remains.

The following test is applicable for *all* secondary alkaloids of Peruvian bark.

Put 1·0 gramme of the quinine salt into a test-tube together with 10·0 grammes water of about 15° C., shake and let it stand for half-an-hour. Then 5·0 cubic centimetres of the filtrate are gently mixed with 7 cubic centimetres of ammonia of about 0·96 specific gravity. If the quinine salt be pure, no precipitate whatever will appear.

The sulphate and the hydrochlorate of quinine must not be coloured by addition of concentrated sulphuric acid in any way. A red tinge indicates salicine, a black one sugar, etc. Both salts must of course be thoroughly soluble in a little water with very diluted sulphuric acid, as well as in common spirit of wine. On a platinum spatula they must burn away without leaving any residue.

SALICYLIC ACID.

Salicylic acid is not only the newest of the febrifuges, but of all therapeutic agents up to this date. It is an agent, too, that is destined to occupy no transitory place in the long list of medicines. Known long ago as one of the component parts of the essential oil of the American *Gaultheria procumbens*, of our *Spiræa ulmaria* and *Monotropa hypopitys*, which oils contain it in the form of salicylic methyl-ether, it was formerly prepared from salicine, the bitter principle of the willow, from whence its name is derived. Salicine is decomposed by saliva or by boiling with diluted acids into saligenin and sugar, and saligenin wants only one atom of oxygen more to become salicine. Subsequently it was successfully produced from carbolic acid. This well-known product of coal-tar has a very close relationship to salicylic acid, as the addition of one equivalent of carbonic acid to the formula of carbolic acid gives that of salicylic as shown in the following diagram:—

$$\underset{\text{(Carbol.)}}{C_6H_6O} + CO_2 = \underset{\text{(Salicyl.)}}{C_7H_6O_3}$$

The process, however, by which this transformation was effected continued to be rather expensive until a few years ago, when Professor Kolbe, of Leipzig, instituted a new and inexpensive process for the purpose, which is essentially as follows. He heats carbolic acid and hydrate of sodium together in a receptacle so contrived that a stream of carbonic acid is made to pass through the mixture, thus inducing the desired union of elements. The product so formed is the salicylate of soda, from which salicylic acid can be easily set free by any stronger mineral acid.

The salicylic acid of commerce is a yellowish white powder, having somewhat the odour of carbolic acid. In order to obtain it quite pure, it is necessary to heat the crude acid in a porcelain vessel covered over with filter-paper. The pure acid sublimes and attaches itself in the form of fine colourless needles upon the paper. One needs to apply the heat very cautiously however, since salicylic acid is decomposed at a higher temperature back into carbolic and carbonic acids.

The pure acid thus prepared has no odour, is of a sweetish taste, dissolves in about 300 parts of water, very readily in alcohol, ether, and chloroform, but most readily in water containing an alkali in solution; with the alkalies presented in this manner, it forms neutral salts.

Three properties possessed by salicylic acid seemed to Prof. Kolbe to give a guarantee that this newly-developed agent could be made to subserve a useful purpose in medical practice, viz.: first, that of its being a powerful antiseptic and antizymotic; secondly, because it can be taken in very large doses without producing symptoms of poisoning; and thirdly, because it remains a long time in the organism, and in its passage through is only partially destroyed.

The addition of a little perchloride of iron (Fe_2Cl_6) to the urine of a man who has an hour or two previously taken a few grains of salicylic acid, causes the appearance of a fine pink colour, the same as that given by mixing solutions of these two substances in pure water.

The first experiments with the new agent under consideration were made in Prof. Thiersch's surgical wards in Leipzig. He began by employing it instead of carbolic acid for the dressing of wounds after Lister's method, and found it to answer all his expectations. Over carbolic acid it possesses for this purpose the undeniable advantages of being less irritating to wounds and of having no disagreeable odour. Very soon afterwards, a number of experiments were made with the internal use of salicylic acid in connection with febrile diseases, principally in the clinic of Basel (Bâle) by Prof. Junnermann and Dr. Buss.

I shall not enter here upon any more minute discussion of the practical applications of salicylic acid, this branch of the subject having been fully treated of by Dr. Ewald, of Berlin, in the March number of this journal. We have now to do rather with the question as to how this agent performs its indisputable antizymotic action.

At first it seemed there could be no doubt but that the influence of salicylic acid in disease was one due entirely to a certain clinical affinity possessed by this agent for the tissues of the body and for the disease-producing ferments invading the same. But then Salkowski[1] suggested that if salicylic acid becomes converted into the neutral salicylate of soda when mixed with a solution of the alkaline carbonate, this change must take place in the blood, and this neutral salt possessing little or no antizymotic power, how would it be possible to explain the antizymotic action of the drug by attributing the same to an energetic affinity of salicylic acid towards cells and ferments? To increase the perplexity, it was very soon found that the neutral salicylate of soda given instead of the acid itself possessed an antipyretic action in every respect as great as the acid, and was without its disadvantages, viz.: that of being almost insoluble in water, of impairing digestion, and irritating the bowels. A sharp discussion arose in consequence, the result of which has been to clear up the difficulties in a certain measure.

As far as my own observations extend, I am convinced that in the discussion upon the application of salicylic acid and the salicylate of soda to therapeutics, the participation of one important factor in affecting the mode of action peculiar to these agents has been allowed altogether to escape attention. Our tissues, as is known, continually produce carbonic acid, of which the blood, notwithstanding its alkalescence, holds a good part in solution, either in a free state or in combinations, from which the CO_2 is constantly ready to separate. *Now this gas, developed absolutely pure, possesses the power of setting free the salicylic acid contained in the salicylate of soda,* a fact the importance of which cannot fail to be apparent as offering an explanation of the paradox above mentioned.

Ether shaken up with a one per cent. solution of the salt leaves behind upon evaporation no appreciable deposit. Another portion of the same solution first treated with CO_2 at the ordinary pressure of the air and in a room of moderate temperature, and then agitated with ether, gives over to the latter the seventh or tenth part of the salicylic acid contained in its salicylate of soda. The same proceeding, if repeated, continues to result in a further liberation of the acid.

The *rationale* of this action is one easily seen into. The salt dissolves very freely in water, in ether but sparingly, and so remains by the former. The behaviour of the acid is itself just the reverse,—it goes over to the ether, and when this evaporates remains behind, partly in the form of elegant crystals, thus showing clearly that the CO_2 has set it free from its previous combination with the soda.

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[1] *Berliner Clin. Wochenschrift*, 1875, No. 22.

Also if one first adds to the solution of the salt a little of the phosphate and carbonate of soda, so as to represent the conditions present in the blood, and then leads in the stream of CO_2, the liberation of the salicylic acid succeeds in a manner equally well marked, only that the quantity set free is somewhat less than otherwise it would have been.

Putrefiable mixtures sufficiently diluted with water, as for instance urine with an equal volume of water, undergo putrefaction when exposed to the air much less readily if previously treated with salicylate of soda and carbonic acid together than after the addition of either of these alone. This difference of action may extend over a period of several weeks. The best plan is to arrange the experiment so that one will have four preparations under observation at a time: of these, No. 1 is to have nothing whatever added to it; No. 2 to be merely impregnated with CO_2; No. 3 with salicylate of soda; and No. 4 to have both this salt and CO_2. Here also we must suppose a liberation of the strong antiseptic to take place which is only overbalanced by the gradual development of ammonia in the liquid.

We see therefore that it is not admissible to designate the salicylate of soda as a substance chemically inactive within the organism. Of a salt which is known to be decomposed by CO_2 no one can *à priori* assert that it passes unchanged through the organism of man, where every tissue with which it is brought in contact by the blood is constantly eliminating carbonic acid in a nascent condition, and in such quantity as to reach an aggregate of 700 grammes in twenty-four hours for an adult.[1]

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[1] This may also hold good for Palli's sulphite and hyposulphite. These salts are immediately decomposed under the influence of CO_2, sulphurous acid being set free. The opposition made by Braun and Bernatzik (*Wiener Med. W.* 1869, No. 100) upon this theoretical point is, therefore, certainly unjustifiable. They assert, namely, that neither in the circulation nor in the secretion of external wounds is it possible for free SO_2 to be developed from these salts, a claim which

For this bearing of the question it is of no consequence *how* the acid appears in the urine. In two cases of quite fresh acid urine I was enabled to isolate crystals of free salicylic acid from the liquid. In the one case Dr. Heubach, my assistant, had taken 1·0 gramme (15·4 grains) of the salicylate of soda, and in the other a patient of the medical clinic had received five grammes of the same preparation. The urine used for these experiments was obtained two hours after the administration of the drug in each case, and the quantities taken were respectively 35 and 50 cubic centimetres (1 and 1½ fluid ounce).

According to Feser and Friedberger it is not practicable to extract salicylic acid from simple blood by means of ether; the blood must first be rendered acid.[1] This also is no proof against the correctness of the above deductions relative to the intra-systemic decomposition of the salicylate of soda, for we do not know in how firm a combination with the albuminoid substances of the blood the salicylic acid may have stood, combinations which ether alone would be powerless to dissolve.

Our animal organism is by no means a simple alkaline combination, such as a solution of soda and basic phosphate of soda in water. Zuntz, who has studied the alkaline action of the blood upon litmus most thoroughly, takes the presence of *free* carbonic acid in the blood of all living animals to be an unequivocal fact. The oxyhæmoglobin performs certain functions of a free acid; and that all tissues actively engaged in destructive assimilation at least do not exhibit an *alkaline* reaction, and that they are in any case charged with carbonic acid, are facts which are partly proven and partly deduced as a necessary consequence from several other established facts.[2]

The behaviour of pyrogallic acid toward the organism also teaches that it is an error to regard this latter as a simple alkaline mixture of elements. Cl. Bernard long ago called

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can only be correct in so far as it may be true, that the acid just developed is at once seized upon by the albumen present. The latter, however, then becomes a different body from what it previously was.

[1] *Archiv für wissensch. und prakt. Thierheilkunde*, 1875, Heft 6.

[2] Compare, among other authorities upon this point, Dubois-Raymond, *Berliner Monatsberichte*, 1859, 288.; Pflüger, *Archives of Physiology*, x. 312; Buchheim, *ibid.* xii. 326.

attention to the unchanged condition in which pyrogallic acid, a substance that oxydises with extreme rapidity in alkaline solutions, circulates in the fluids of the body. Under the guidance of Hoppe-Seyler,[1] Jüdell has further shown that pyrogallic acid taken by the human subject in a quantity of 0·5 gramme (8 grains) appears after two hours abundantly in the urine.

This behaviour would seem quite unaccountable according to the unconditional alkalescence theory, but it is easy to show by experiment what is at least one of the causes of it. Thus, if one makes a solution of pyrogallic acid alkaline, divides the whole into two parts, and leads a stream of CO_2 through one of them, the well-known rapid browning will then be held energetically in check, even though free access of the air be secured by keeping up constant ventilation over both preparations. For the rest, and to avoid the objection that the carbonic acid which escapes hangs upon the surface of the liquid, preventing the O_2 of the air from reaching it, and hence the retarded oxidation, let the one half of the pyrogallic acid solution be augmented by the addition of carbonate of soda one part, the other with bicarbonate of soda two parts, at a low temperature. The effect is similar to that obtained in the first experiment; for while the brown colour appears in the one preparation as soon as the Na_2CO_3 is added, it is only slightly indicated in the other, where an excess of CO_2 is present. The same difference of effects manifests itself if into one portion of a solution of pyrogallic acid that has been made alkaline one immediately leads a stream of air, while through another portion of the same solution a second current, consisting of oxygen 21 volumes per cent. and carbonic acid 79 volumes per cent., is made to pass. Here the quantity of O_2 presented is similar on both sides, while the effects are quite dissimilar.

Nothing necessitates us to regard the decomposition of the salicylate of soda as taking place in the blood: it is rather to be looked for in the acid-producing tissues. Nor is it by any means urgent upon us to locate the seat of the infective processes in the blood; on the contrary, the ferments which excite disease invade our system from without through the medium of the lymphatic vessels, as is shown by the manifold processes of inoculation with which we are acquainted. The very circumstance of the persistent swelling of the adenoid organs in infectious fevers points to these as the constant centres of the disease. But immediately after death the reaction of the lymphatic glands is *not* alkaline. Further, it is known what a quantity of acid substances the living spleen produces.

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[1] *Untersuchungen*, 1868, 422.

In attempting to analyse the antipyretic action of salicylic acid one need not confine oneself to the circumscribed idea of antisepsis. We know that free salicylic acid exerts an action upon an infinite variety of ferments. It also cures such diseases as are characterised by an increased production of heat without being of a putrefactive nature. In the one case as in the other, it will affect albuminoid substances, toward which it may have a special affinity, either altering their nature or checking their activity as ferments. That an influence upon protoplasm belongs even to the neutral salicylate of soda was demonstrated on certain bacteria under the experienced guidance of Dragendorff.[1] The power of this salt to paralyse the development of bacteria shows itself to be greater than that of either phenol, quinine, boracic acid, or alcohol, and almost one third as great as that of free salicylic acid. Therefore, even setting aside the possibility of salicylic acid being set free from it in the tissues, a compensation for the inferior activity of the sodium salt would be offered in the much more rapid absorption of the latter into the system.

The view that the salicylate of soda exerts its antipyretic power through the medium of the heart or the respiration, seems to me to be quite unsupported. It is an easy matter to show that dozens of poisons which are absolutely powerless against fever possess the property of causing a depression in both the circulating and respiratory systems. The temperature of the body *can be* influenced by these poisons, particularly if the action of any one of them is driven so far as to bring about collapse. In fact, I scarcely know how one would proceed in order to get actually no diminution of temperature with such powerful agents as these. With salicylic acid, however, or its salt exhibited in such doses as those commonly given with good effect, there can be no talk of collapse, except in cases where a spontaneous and sudden decline of temperature coincides with the reduction effected by the drug.

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[1] L. Bucholtz, *Arch. für exper. Path. u. Pharmak.* iv. 32 and 80. (1875.)

Ewald expressly asserts that an action of salicylic acid upon the pulse and respiration in both forms, as tried in Frerichs' clinic for antipyretic purposes, was almost entirely absent. Riess, Buss, and other observers give reports agreeing essentially with this. If the reports of these observers are not sufficient to completely negative the claim in question, it may still be said that nothing more than an *inconstant* primary influence upon both systems is unanimously spoken of. The same holds good with respect to an influence upon the perspiration. In a case in Wunderlich's clinic at Leipzig the temperature went down after salicylic acid from 105° to 94° F., without any visible alteration in the skin.[1]

Salicylic acid possesses in all outward respects a resemblance to quinine. It combats the malarial poisoning (Senator, Buss) —although less surely and effectually than quinine—during the period of apyrexia, where, as is well known, neither the pulse nor respiration necessarily need present the slightest abnormity. It is, like quinine, a powerful antizymotic, which can be introduced into the organism in large quantities, circulates there a considerable length of time, and is given off again—at least partially—in an unchanged condition. Even the ringing in the ears and the slight deafness characteristic of cinchonism are not wanting in connection with the medication with salicylic acid. A complete agreement between its behaviour and that of quinine toward certain disease-producing agencies, known, as must be confessed, only by their effects, does not exist. This we have already seen in speaking of intermittent fever. Acute articular rheumatism, a disease in which quinine avails so little and salicylic acid so much, furnishes a second and converse example. Considering the chemical dissimilarity of the two substances, it is not to be expected that there should exist a similarity between their modes of action on more than the general points involved. This general resemblance, however, is unquestionably present, and so we shall have to seek an explanation of the manner in which their therapeutic action is exerted in the same channels.

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[1] *Archiv für Heilkunde*, 1876, p. 378.

I cannot dismiss the subject of antipyretics without referring to the class of emetics and purgatives which often yield decided antipyretic results. But as their mode of action on the whole is so indirect, and their effects as a rule so transitory, they are scarcely entitled to be reckoned as true febrifuges. And as far as I know, there has been as yet but little done experimentally in the way of explaining the exact method of their antipyretic power. Perhaps the most striking instance of a purgative acting as a febrifuge is that of calomel, as given with such marked success in incipient typhoid fever. Here it is almost certain that the calomel is changed into perchloride of mercury within the system, and this latter being one of the most powerful antizymotics that exists, would probably act directly upon the disease-producing ferments at work in the intestines.[1]

[1] In the former article, twelfth line from the bottom of p. 283, instead of *although* read *because*.

CASES OF INSANITY RELIEVED BY ACUTE DISEASE.

BY GEORGE H. SAVAGE, M.D. (LOND.)

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It has been the idea for ages that insanity might be cured by sudden shocks, and this belief led in former times to great abuses. I am inclined to think, that though the rough treatment did serious harm in some cases, yet it cured others that now-a-days remain uncured. Though we no longer make use of these rough and ready means of cure, we are often struck by the temporary or permanent relief of insanity following immediately on an acute bodily illness.

During the past year I have seen several such cases, and I think them noteworthy; and, with Dr. W. Rhys Williams's permission, subjoin particulars. It is rare to see any improvement follow on a sudden mental shock, such as the receipt of good news; thus, the woman who is under the delusion that her children have been killed is not cured when she sees them alive. On the other hand, continuous mental action of a definite and purposive kind, if suddenly started, may result in ultimate cure; this is exhibited in cases that have had a chance of escape which they have seized. I have seen several cases that, after partial recovery, remained at a standstill till they attempted escape, and having succeeded in getting away, and, finding their way home again, being caught and brought back, have from that time begun steadily to improve, and have been discharged cured.

In many cases the condition of partial cure is to be dreaded; the balance is nearly adjusted, but it is very hard to find just the proper force to make it turn towards health. I have found in such a condition the stimulus of a shower bath or of galvanism useful.

Many cases are on record in which the attempt to commit suicide has shaken the balance back to a normal equilibrium. Cases too are on record, and I have seen at least one, in which recovery followed the opening of an abscess. Of a similar nature, perhaps, is the improvement in some cases of mental disease when active mischief is present in the lungs. Thus we have most of us seen cases of insanity relieved as phthisis progressed. Again, some insane persons recover sanity in the act of death. I have the notes of one case of melancholia that began to improve after an epileptic fit, this being the only fit she has had. This patient recovered permanently.

I purpose now giving short notes of five cases. These cases differed much in their general symptoms, and agreed only in the cure following on an acute illness. Two of the patients were suffering from acute mania, two were melancholic, and one was in a state of partial dementia following the exhaustion of parturition. The cure was not permanent in all: one being only sane as long as erysipelas was present, and again becoming melancholy on its cure. In the other four, the illness was the starting-point of the recovery.

CASE I.

Melancholia passing into partial Dementia following child-birth.

Kate P., aged 24, married, delivered of her first child in September, 1875. A very severe labour was terminated by use of forceps; there was laceration of the perineum, and considerable hæmorrhage. She suckled her child, but had a scanty supply of milk. Five weeks after delivery she fell into a lethargic state, and had delusions that she was going to be poisoned. She had been sleepless before these symptoms came on. She refused food. On admission she was extremely weak and anæmic, in a state of partial dementia. She took no interest in her child.

For six weeks, though she took food fairly well, she made no mental improvement, and would rarely speak, and then only in a whisper. She was treated with a liberal diet and tonics. Seven weeks after admission she had most severe pains in lower part of abdomen with constant vomiting. This continued for three days, drugs and other remedies giving no relief. The case becoming now urgent, Dr. Gervis of St. Thomas's Hospital saw her, and diagnosed a retro-uterine hæmatocele.

From the very onset of the pain and vomiting the patient changed completely in her mental state, and instead of being dull and lethargic, became bright and pleasant, submitting to all necessary examinations and treatment willingly. She too exhibited affectionate interest in her husband and child. She was confined to bed for five weeks, and is now convalescing physically and is well mentally. She never has had any relapse since the development of the hæmatocele.

The two following cases are of interest, as, in both, the dental nerve was the one irritated. It is noteworthy that French writers have several times referred to irritation of the fifth nerve in its dental branch as a cause of insanity.

CASE II.

Acute Mania of six months' duration improving after toothache and extraction of a tooth.

R. P., aged 22, single, a farmer, with strong family taint of insanity. First attack of insanity in May 1875. He suddenly took to riding madly about the country without his coat and waistcoat. From May till November he was one of the most noisy, destructive, and untidy cases of mania we had. He was almost constantly excited, and if for a day or two he was quiet, he was sullen and more dangerous.

In the middle of November he complained of very severe toothache, that caused him to be sleepless; he bore this for two or three days, when I removed a stump; there was suppuration at the root of the fangs. From this time the patient steadily improved, and was discharged on leave in the middle of December, and at the end of the month went home well.

CASE III.

Acute Mania of four months' duration; recovery after inflammation of the jaw.

H. W., married, aged 34 years, brother insane. Has been intemperate of late. Was delivered six months before admission of her fifth child. Has not menstruated since. Spat up some blood three months ago. Was admitted early in September suffering from very acute mania. She was noisy, violent, and obscene. This state of excitement was so extreme that she had to be removed to the worst gallery.

She had, as a complication, inflammation and contraction of her right knee, which was straightened and put up under chloroform. She succeeded in tearing off plaster of paris bandages twice, and was only mastered by a silica bandage. Her general health improved, but mentally she was as bad as she could be. This continued till January 20, 1876, when she complained of great pain, with swelling and redness about her right lower maxilla. She had some bad teeth, but did not complain of toothache; the pain and swelling increased, and at the same time she became quiet and reasonable. She says she cannot remember much of her state of excitement. The swelling of the face subsided, and she is keeping quite well. There is a possible second cause of recovery here, as she is pregnant, and now says she felt "quickening" about ten days before her recovery. The recovery was coincident with the pain and swelling of her face, and so I prefer to consider that as the active cause of her recovery.

CASE IV.

Temporary recovery from Melancholia during Erysipelas.

Caroline S., single, aged 52, a dwarfish woman. No insane relations. Admitted September, 1875, suffering from delusions that she was being poisoned and being led into crimes. She threatened suicide, was obstinate in refusing food, and had once or twice to be fed by the tube. She continued thus till January 15, 1876, when, on a self-inflicted injury to the head, a sharp attack of erysipelas of head and face supervened. This lasted a week, and during this time she was docile and quiet, took her food well and without scruple, and was reasonable in every way. She had no delirium.

On recovering from the erysipelas she again developed her delusions, and is in much the same state as before the illness, save that she takes her food better.

CASE V.

Melancholia, symptoms less while suffering from bowel obstruction due to foreign bodies swallowed.

As Dr. Williams purposes reporting this case in full, I merely give a few particulars.

Emily C., single, aged 25, with distant taint of insanity. This is the first attack. Admitted November 1875. Very suicidal; thinks she has sinned against the Holy Ghost. She improved for nearly a month, and then took to swallowing corks, buttons, and all sorts of things. She passed many of these articles, but after some days there were signs of obstruction. She had severe vomiting, and complained of severe abdominal pain, so that for several nights she was sleepless. During this period of pain she was free from delusions; she passed several more foreign bodies, including a mass of horse-hair, and then, on freedom from pain, was again depressed. She was removed to another gallery, and has since steadily improved, and though an anxious case, we trust her recovery this time will be permanent.

These cases are interesting from many points, but I think it will be wiser to leave them in the hands of the reader with very few other remarks.

It seems as if severe pain will do good, either by rousing into energy, or occupying the mind and getting it out of its deepening grooves; or the local irritation of a nerve, such as the fifth, so nearly connected with the brain, may alter the cerebral circulation. I have, by galvanism through the head, increased the force of the radial pulse on several occasions, thus, I think, showing an influence may be caused in the central circulation by irritation of the extremities. At the suggestion of a friend I intend galvanizing the dental branch in a series of cases; I hope to give the results in some future paper.

NOTES ON THE USE OF OPIUM AND ITS ALKALOID MORPHIA IN THE TREATMENT OF CERTAIN FORMS OF INSANITY.

BY BREWARD NEAL, M.R.C.P. EDIN.

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WOOD, in his valuable Treatise on Therapeutics, gives the following description of the physiological action of opium:—

"When opium is taken in such dose as to produce its mildest physiological effects, it exerts a quieting influence, inducing a peculiar dreamy condition, very generally a feeling of *bienfaisance*,—during which images and ideas float before the mind, and by their endless and effortless repetition shorten the time, which seems to lose itself in rest. It is commonly asserted that there is a stage of the action of opium in which the activity of the mental faculties is exalted. This may be so in some persons, and especially in those who have accustomed themselves to the use of the drug as a stimulant; but my experience is that in those who do not habitually take opium true mental power is, during all the stages of the action of the drug, diminished rather than increased. The state induced is rather the fabled calm of the lotus-eater than the energetic activity of production. Even in those who are accustomed to the use of opium as an aid to work, I think it is the imagination rather than the reasoning faculties which is excited by it. After a length of time, varying according to the idiosyncrasies of the patient and the dose of the drug, the condition which has been noted gradually passes into sleep,—either light and dreamful, or natural, or heavy and deepening into stupor, according to the amount of the drug ingested.

"After awakening from the sleep the patient may return at once to his normal condition, but very often experiences a state of depression, as shown by languor, a little headache, nausea, or even vomiting, which may last for some hours.

"After very large doses the first stage of the action of opium is very short, or it may be entirely wanting, sleep coming on almost at once. Thus, I have seen deep coma produced in three minutes by a hypodermic injection of morphia. The symptoms of the second stage of opium-poisoning closely resemble those of congestion of the brain: the pupils are strongly contracted, the face more or less suffused, often deeply cyanosed; the pulse full, slow and strong, the skin generally dry and warm; the respiration slow and deep, and it may be stertorous; unconsciousness is apparently complete, but very generally the subject can be aroused by violent shaking, or by shouting in his ear, but relapses at once when left to himself. When the patient is aroused, the respirations become more rapid, and the skin often regains almost at once its normal colour. Death very rarely occurs during the second stage of opium-poisoning. When the symptoms do not gradually ameliorate, the third stage, that of prostration, is developed. The coma is now profound, and to arouse the patient may be impossible; the pupils are absolutely contracted, or, as death approaches, are widely dilated; the respirations are slow, feeble, and imperfect, and often interrupted by intervals of death-like quiet; the countenance is at once pallid and cyanosed, the pulse grows more and more rapid, and more and more feeble; the skin is cold and moist, finally covered with a clammy sweat. Even yet the patient may recover; if he do so, the return to life is most gradual; if he do not, death occurs generally by failure of the respiration, but amid an almost complete extinguishment of the vital functions. In man the circulatory phenomena are a slight primary evanescent acceleration of the pulse-rate, succeeded by slowing and increased fulness and force of the pulse, which is followed by a return to the normal pulse, or a great increase of rapidity and loss of strength during the third stage."

In the exhibition of opium or morphia, their physiological action should be kept in view, and the individual characteristics and requirements of the case to be treated arrived at before a scientific application of the drug can be put in practice. In cases of insanity where opiates are indicated, morphia, either in the form of acetate or hydrochlorate, seems preferable to the other pharmacopœial preparations, being more certain in its action, and producing less disturbance of the digestive functions. Two other preparations (non-officinal), the Liquor opii sedativus and the anodyne tincture, or nepenthe, are valuable, especially in cases of melancholia.

In cases of acute mania characterised by continuous restlessness, incoherence, and sleeplessness, and often with mischievous destructive propensities, delusions of a happy, gay nature—the patient however in many cases capable of answering one or two questions rationally and coherently when pulled up, but speedily becoming lost again in the maze of his disordered imagination, and hurried on from subject to subject by the intensity and abundance of his ideas—the result probably of active cerebral congestion, and accompanied with a bounding pulse, tumultuous heart, and other evidences of vascular excitement, the exhibition of morphia is indicated, and when combined with a cardiac sedative, as tartrate of antimony, is often administered with the best results. The tartrate of antimony in such cases seems beneficial by diminishing the cardiac excitation and shortening the accelerative action of the morphia, and thus hastening its second, or sedative stage; it should however be given only in small doses, and never does good if it produces sickness. Another useful combination where a speedy hypnotic is desired, to be followed by more continuous sleep, is the hydrate of chloral with morphia.

In those cases of maniacal delirium where the patient is unconscious of everything going on and unable to recognise those around him, and where he cannot be withdrawn even for a second from his chaotic ravings, such mental symptoms being accompanied with great heat of skin, a loaded tongue, sordes on the teeth and lips, offensive breath and confined bowels, and refusal of food, etc., it will be found as a rule that morphia is inadmissible, in fact in many of such cases in the early stages it seems to do positive harm, and increase cerebral irritation and consequent excitement. In such cases other sedatives; as Succus conii, tincture of hyoscyamus, hydrate of chloral, etc., and the use of prolonged hot baths and cold douche to the head, or in some the calmative action of a darkened room, and in others active out-door exercise is more beneficial; but should the acute stage pass without a return to sanity, and the patient still remain restless, incoherent, and irrational, in fact in a condition allied to that of the ordinary acute mania (*sine* delirium), morphia of all sedatives is most useful, either alone or in combination with tartrate of antimony or hydrate of chloral, according to the necessity or not of giving a vascular depressant. In those cases of mental disease characterised by impulsive outbursts of violence and other turbulent acts, occurring from time to time without any provocation on the part of those attacked, morphia often produces beneficial results, and seems to create in the patient self-control, or rather enables the patient to exercise that power of self-control which is lost or latent in such cases of mental turbulence described in what Hammond calls the volitional form of insanity. These volitional defects frequently occur in young women, and are often associated with difficulties of menstruation, irregularity of the bowels, nympho-mania, etc. What explanation of these phenomena in connection with impulsive acts of violence can we give? The following, I think, may in some measure be the connecting link between the catamenial period and the pathological condition producing outbursts of an impulsive nature. The sexual apparatus at the time of menstruation becomes congested, and as a result an exaltation of function occurs similar to the inhibitory acts described in a paper in the W. R. Asylum Reports for 1875, only with this difference that an augmentation of action is evoked instead of a restraining influence. The stimulus in this case is transmitted by the sympathetic from the reproductive organs to the special cerebral convolutions (probably the posterior lobes, for, according to Van der Kolk, in all cases of sympathetic insanity proceeding from the sexual organs, disease of this portion of the brain and its membranes is noticed) involved in the production of amatory feeling, which evolves an uncontrollable sensuality, and the inability to gratify and appease this consuming desire seems to evoke and result in paroxysms of violence and anger, apparently a kind of safety-valve action to the pent-up emotions. Having disposed of those cases of mental disturbance which derive benefit from taking morphia classified under the head of mania, I will now direct attention to another, and if anything, a more fruitful source of good results than the former forms of mental disease. The class of mental derangement I refer to is melancholia in its various phases, and frequently connected with an anæmic state of the brain, and passive venous congestion, weak, languid circulation, deficient dermal action (the skin being frequently dry and harsh, sometimes relaxed and flabby, and generally cold), inactivity and irregularity of the bowels always; the tongue varies in its appearance, sometimes furred, at others pale, flabby, and indented at the edges. The melancholic as a rule are not disposed to take food without considerable firmness being used to induce them to do so, and in some artificial feeding is requisite. They refuse either from apprehensive fears about poison, or from a suicidal intention. There are however exceptions; for instance, I have known patients of this class eat enormously, and then deny that they have done so: some will eat when they think they are unobserved. Other symptoms are a troubled and distressed expression of countenance, restless and gloomy manner, and in some instances an inability to sit quietly even for a few minutes. Some rock themselves to and fro, others wring their hands, and nearly all are only too glad to dilate upon and exaggerate to the sympathising listener the most minute details of their wretched feelings and hopeless condition, and in some recreation or employment is impossible, whereas in other cases any kind of mechanical employment is eagerly sought after. For the successful treatment of such symptoms opium or morphia should be carefully administered in small and frequent doses, for the purpose of obtaining a prolonged stimulative action of the drug, which, by increasing the blood pressure, accelerates the lingering arterial circulation, and augments the supply of nutriment to the famishing cerebral cells.

With regard to the hypodermic use of morphia, the following advantages may be gained by its adoption.

1. In some few cases where rapid action is required, as in the case of those patients who are in a violent state of noisy excitement, and in whom the administration of medicine by the mouth could only be effected by the use of great force.

2. In certain cases where from irritability of the stomach it cannot be retained, although the hypodermic method is not entirely free from objection on this point.

The above being the advantages, it must not be forgotten that it has its disadvantages. It requires care, on account of the danger of injecting a vein, although after an extended experience of its use I have seen only one case attended with dangerous symptoms. This was one of maniacal excitement in an epileptic, and in whom I have reason to think the narcotism was intensified by the supervention of an epileptic convulsion after the administration of the drug. The other objections are that it is somewhat painful and alarming to some patients (but in the majority of cases their condition is such that no difficulty of that kind is experienced), and in addition it occasionally produces local inflammation, ulcerations, etc.

To get the curative powers of these agents, in some cases they require continuous and careful administration for considerable periods, and where they produce the greatest benefit a toleration of the remedy seems to be established, and I have no hesitation in stating that in curative virtues morphia of all sedatives stands alone, and without doubt unrivalled in promoting an alteration and revival of healthy processes in the deranged or morbidly excited brain. In giving the foregoing short account of some of the uses of opium and morphia I have abstained from stating anything more than general and leading indications for their use; each case for treatment will present special symptoms, and the varying conditions, both mental and physical, of the individual must be relieved by the application of therapeutical means according to the requirements of the case: some will take with advantage quinine or other nervine tonic, either alone or in combination with phosphate or perchloride of iron, strychnia, or some simple vegetable bitter, some chalybeates alone, mineral acids, stimulants, or anti-spasmodics, such as æther, ammonia, and camphor, with a judicious use of cathartics, aloes, etc., and occasionally counter irritation, and hot, cold, or shower baths; and with all this great care should be exercised as regards what is called the moral treatment of the insane. I need hardly say it plays a most important part, for without it the application of the most efficacious remedies would prove unavailing. Added to this an appropriate, adequate, and varied dietary is also most essential, and alcohol in its various forms may be included in this category.

Reviews.

Surgery: its Principles and Practice. By J. Holmes, M.A., Surgeon to St. George's Hospital, &c. London: Smith, Elder, and Co.

EVER since it was rumoured that Mr. Holmes was preparing a handbook of Surgery, the work has been anticipated with considerable interest. Familiar to an extent that is probably unrivalled with the literature of his subject: a lecturer on Surgery at a Hospital which affords an ample field for practical experience: and the able and expeditious editor of, and large contributor to, the *System of Surgery*, he seemed to be completely armed for such an undertaking. Besides, his are just that order of mind and just that kind of temperament, that the occasion required. He has a vigorous and cultivated understanding, equally proof against an egotistical indulgence in pet theories and dogmas, and against that confusion between principle and detail, between the essential and the secondary, that so often mars otherwise good work. He is, too, always the man of high and honourable feeling, always above personal rivalry, always more ready to acknowledge the worth of others than to parade his own performances. True, he is perhaps a trifle caustic, a trifle even contemptuous in places; but is this a fault if it is not carried too far? A sledge-hammer is an admirable weapon, if it is used with discretion. We see no ground for complaint against Mr. Holmes in this respect: indeed, considering that he always was (as *Punch* some time ago said of Mr. Gladstone, when he had discomfited a distinguished political opponent) an uncommon strong boy, we think he has shown commendable self-restraint. Before proceeding to notice particular chapters, we may state that Mr. Holmes has completed his labours in a manner that is highly satisfactory; and we may at the same time include his publishers in our very favourable verdict. As to size and type, the book corresponds with Gray's *Anatomy*, of which Mr. Holmes is the editor. It thus forms a much more convenient volume than many of the current text-books, whose letterpress is so small and close that they cannot be read with any sort of comfort, and whose shape is so nearly that of a cube that they won't lie open till they are all to pieces. The illustrations are numerous, and many are excellent; some, however, are very indifferent indeed, and tempt us to repeat the old lady's remark respecting the annotations of the *Pilgrim's Progress,* to the effect that she could quite understand the text, and hoped soon to know something about the notes. Could Mr. Holmes himself tell us, if he did not know the preparations, what the figures on pp. 274 and 623 were meant for? We thought they referred to the uterine decidua till we read their descriptions, where we learnt that the one represented a tranverse fracture of a patella, the other a fistula in ano. Whether the figure on p. 298 would most puzzle a man who did or a man who did not know his bones we cannot say. Fig. 249 on p. 519 we venture to think will find no place in the author's second edition, and so, let us be thankful, we shall soon get rid of it. It is a pity that these illustrations and some others are so poor, for the remainder are well drawn, and by their number as well as by their general excellence add very largely to the value of the work.

After a sketch of inflammation and other kindred processes, which, although necessarily very condensed, is yet sufficient for the purpose of placing the student *au courant* with the principal doctrines of the time, Mr. Holmes turns to Hæmorrhage and its treatment, of which his account is a model of clearness, conciseness, and completeness. The author then treats of the general pathology of fractures and dislocations; and then gives an excellent description of injuries of the skull. He has doubtless earned the hearty thanks of many a beginner by the way in which, in the following remarks, he breaks down the old student's puzzle of fracture by *contre-coup* (p. 138). "Strictly speaking the term ought to mean that the skull being struck at a given point, a fracture occurs at the point diametrically opposite, while no force is applied to the skull in the latter situation. This appears to be quite unproved. If the vertex be struck, fracture often takes place at the base and not at the vertex—or if one side of the head be struck, fracture often takes place at the other: but in the former case the injury to the base is due to the skull having been made to impinge forcibly on the occipital condyles, and in the latter, as far as I have been able to discover, it has always been the case that the opposite side of the skull has been driven against an obstacle; so that both are instances really of direct force. Again, when a blow is struck on one side of the vertex of the skull, the opposite side of the base of the brain is often lacerated, but this is obviously caused by the brain having been driven down on the rough base of the skull. The term is, therefore, speaking accurately, quite incorrect; but it may be retained, as having now come into general use to express the fact that a blow on one part of the skull often produces its main result at the other end of the axis of the cranium, a fact which is of considerable practical importance." In his chapter on injuries of the spine, Mr. Holmes, on very sufficient grounds, joins Mr. Le Gros Clark in withholding his support from the operation of trephining the vertebral column for fracture. He gives a *résumé* of our present knowledge respecting "spinal concussion," and the effects of railway injuries, and in referring to the contradictory evidence that eminent surgeons often give in courts of law—evidence that is so vexatious to the profession, so amusing to the lawyers, so suggestive of grave doubts to the suspicious among the general public—points out what is probably the only way in which these occurrences may be avoided—namely, by a "deliberate examination (of the case) by a medical commission authorized to avail themselves of all necessary opportunities for forming an unbiassed judgment." The section on injuries of the lower extremity, including fractures and dislocations, is clear, judicious, and well up to the present day. We observe, however, that Mr. Holmes, without strongly recommending them, yet speaks favourably of Malgaigne's hooks for the treatment of fractured patella. We wish we could believe our authority sufficiently weighty to overrule his upon this point. These hooks we think are a barbarous invention. (Mr. Holmes speaks of sticking them either into plaster, or into the skin and fibrous textures of the patient—allusion is here made only to the second method.) Their use is certainly not free from serious danger. We have seen two cases in which they produced widespread suppuration in the limb (Mr. Holmes with his usual candour alludes to similar accidents), and we greatly doubt whether they yield better results than are constantly obtained by methods which are completely free from danger. Some years ago we examined at St. George's Hospital several cases then under treatment with the hooks, and we failed to see that anything was gained by their use—certainly there was no evidence that they secured bony union of the fractures. The chapter on Tumours is one of the most useful in the volume. On this subject the pathology of the hour is sometimes not only at issue with clinical observations, but, if we compare the doctrines of some of its leading exponents, at issue also with itself. In its present transitional state, many points must be passed lightly over. But Mr. Holmes has, we think, succeeded as nearly as is possible in leading his readers to firm ground, and a clear atmosphere, and to a standpoint from which they may best harmonize pathological research with the teachings of clinical observations. For the illustration of this section a number of extremely good woodcuts are introduced.

Mr. Holmes is so well known as an authority on diseases of the arteries, on aneurism, and on diseases of the bones, that we need make no extracts from his chapters on these subjects to prove their value. Our readers, however, will find them very interesting and very instructive. Mr. Brudenell Carter and Mr. Dalby have respectively furnished the sections on the Eye and the Ear, and as Mr. Holmes has placed nearly a hundred pages at the disposal of these very competent authorities, the practitioner will find in their contributions valuable directions for the management of cases. Here, though we have done Mr. Holmes but very scanty justice, we must stop. We can strongly recommend his volume alike to the student still in his teens and to the busy practitioner: the former may learn from it, so far as books can teach him, both the science and the art of Surgery; the latter will find it a concise, trustworthy, well-written, and admirably arranged guide in all the various difficulties and emergencies of every-day work.

Elements of Human Physiology. By Dr. L. Hermann, Professor of Physiology in the University of Zürich. Translated from the fifth edition by Arthur Gamgee, M.D., F.R.S., Brackenbury Professor of Physiology and Histology in the Owens College, Manchester. 8vo. pp. 587. London: Smith, Elder, and Co.

THE rapid appearance of successive editions of this work in Germany, and its translation a few years ago into French, sufficiently attest the favour with which it is regarded on the Continent, and we heartily welcome it in its English dress. The clearness and conciseness of the author's style, as well as the completeness, accuracy, and order of his statements, fully justify the preference accorded to his work in Germany, and the translator has succeeded in giving us a rendering worthy of the original. Every now and again we come upon some turn of expression which reminds us that the book has not been originally written in English, but these are not numerous, and they in no way impair the sense. There is one curious printer's error on the title-page. In the original the work is said to be by Dr. L. Hermann, but in the translation the first word has been mutilated and the author furnished with an initial which does not belong to him, so that his name stands as D. L. Hermann. As a complete work of reference neither this nor any other work comes up to the last edition of *Carpenter's Physiology*, edited by Power, but as a book from which a student or practitioner may acquire a thorough knowledge of all the main facts and most of the details of the science of which it treats it stands unequalled.

Health in the House. By Catharine M. Buckton. 8vo. pp. 207. London: Longmans.

THIS little work, like the preceding, treats of physiology, but in a very different way. It consists of twenty-five lectures on elementary physiology in its application to the daily wants of man and animals, delivered to the wives and children of working men in Leeds and Saltaire. The authoress gives a very popular account of the various functions of the body, and shows why close rooms are hurtful, why tight lacing is injurious, and swinging a child by the arms or boxing its ears is dangerous. The hints she gives on regimen and the directions for cooking are very good. This book ought to have a wide circulation, and if the knowledge contained in it were generally diffused and acted upon there would be much less sickness and premature death amongst the classes for whose benefit it is specially intended.

The Student's Guide to Dental Anatomy and Surgery. By Henry Sewell. London: J. A. Churchill.

FOR some few years past there has been issued from the medical press a series of manuals entitled *Student's Guides;* the subjects contained in them are necessarily compiled in a condensed form, and it is to be feared that with many the process of condensation is carried too far, so that they become merely useful as expanded indexes, and as such are of value to the advanced student only, thus defeating the object of the projectors of these manuals. As we comprehend it, they should be of benefit to pupils commencing their studies. This we are afraid, in spite of the good intentions expressed by the author in the preface, has taken place in the *Student's Guide to Dental Anatomy and Surgery,* by Mr. Henry Sewell. However, the book is fairly and intelligently written, and we may hope that when in a future edition the defect of the meagreness of some of its chapters is remedied it will really become a useful work.

Clinic of the Month.

How to Cure a Cold in the Head.—Dr. David Ferrier remarks, that though a cold in the head gives rise to much discomfort and uneasiness, it is not usually considered grave enough to necessitate professional advice; and the unfortunate victim of nasal catarrh, with watery eyes, running nostrils, sneezing, and nasal speech, is more often regarded as a subject of ridicule than of sympathy or commiseration. Being lately threatened with a cold in the head, and the prospect of serious inconvenience to public speaking next day, Dr. Ferrier endeavoured to devise some plan of treatment more speedy than the usual one of "sudorifics and lying in bed." Having succeeded beyond his expectations in his own as well as in the case of others to whom he recommended the treatment, he offers it in the hope that it may prove equally efficacious in the hands of others. The symptoms being those of acute catarrh of the nasal mucous membrane, the treatment which seemed most likely to succeed was that which Dr. Ferrier has always found most efficacious in acute catarrh of the gastric mucous membranes. In the acute catarrh of alcoholism, accompanied with profuse secretion of mucus, which is often vomited up in large quantities almost without effort, as well as in the more chronic forms of gastric catarrh, bismuth alone, or in combination with morphia, acts almost like a specific. On the same principle the topical application of bismuth to the nasal mucous membrane seemed the plan most likely to be followed by beneficial results. On the evening in question Dr. Ferrier began to suffer with the symptoms of cold in the head—irritation of the nostrils, sneezing, watering of the eyes, and commencing flow of the mucous secretion. Having some trisnitrate of bismuth at hand, he took repeated pinches of it in the form of snuff, inhaling it strongly, so as to carry it well into the interior of the nostrils. In a short time the tickling in the nostrils and sneezing ceased, and next morning all traces of coryza had completely disappeared. Bismuth alone, therefore, proved quite successful, but it is better in combination with the ingredients in the following formula. Bismuth by itself is rather heavy, and not easily inhaled, and it is, moreover, necessary that it should form a coating on the mucous membrane. It is, therefore, advisable to combine it with pulv. acaciæ, which renders the bulk larger, and the powder more easily inhaled, while the secretion of the nostrils causes the formation of an adherent mucilaginous coating, of itself a great sedative of an irritated surface. The sedative effect is greatly strengthened by the addition of a small quantity of hydrochlorate of morphia, which speedily allays the feeling of irritation, and aids in putting a stop to the reflex secretion of tears. The formula which is on the whole the most suitable combination of the ingredients of the snuff is as follows: —Hydrochlorate of morphia, two grains; acacia powder, two drachms; trisnitrate of bismuth, six drachms. As this is neither an errhine nor a sternutatory, but rather the opposite, it may be termed an anti-errhine or anti-sternutatory powder. Of this powder one-quarter to one-half may be taken as snuff in the course of the twenty-four hours. The inhalations ought to be commenced as soon as the symptoms of coryza begin to show themselves, and should be used frequently at first, so as to keep the interior of the nostrils constantly well coated. Each time the nostrils are cleared another pinch should be taken, either in the ordinary manner, or by a method more efficacious and less wasteful of using a small gutter of paper or a "snuff-spoon," placing it just within the nostril, and sniffing up forcibly so as to carry it well within. Some of the snuff usually finds its way into the pharynx, and acts as a good topical appliance should there also be pharyngeal catarrh. The powder causes scarcely any perceptible sensation. A slight smarting may occur if the mucous membrane is much irritated and inflamed, but it rapidly disappears. After a few sniffs of the powder a perceptible amelioration of the symptoms ensues, and in the course of a few hours, the powder being inhaled from time to time, all the symptoms may have entirely disappeared. (*Lancet*, April 8, 1876.)

The Treatment of Severe Sprains.—Mr. Sampson Gamgee observes that severe sprains are often serious fractures, though no bone be broken, or only a bit may be chipped off; the ligaments and fasciæ are ruptured, blood being extravasated into the joints, into the sheaths of tendons, and for some distance not unfrequently between the layers of muscles. The swelling is great, the pain intense. The orthodox treatment by leeches and fomentations is valueless compared with circular compression and perfect immobilization. He quotes a passage from Mr. Holmes's recent work on Surgery to the effect that antiphlogistic treatment should be employed, but maintains that not only can the patient bear well-applied pressure from the first, however great the swelling and acute the pain, but it may be laid down as a general rule, to which he has never seen an exception, that in severe sprains effusion is most surely checked, and once it has occurred its absorption is most rapidly promoted, while pain is most effectually relieved by pressure and immobilization. He thinks it is as true now as when Velpeau taught it, that "compression is the sovereign resolvent in contusions with infiltration and swelling." Mr. Gamgee illustrates these observations by giving the details of a case of an elderly gentleman who had sprained his ankle in going over a ploughed field. The joint became hot and exquisitely painful. The circumference over the heel exceeded that of the corresponding sound joint by nearly an inch and a half. It had been proposed that an opening should be made to let out matter that was believed to have formed; but Mr. Gamgee proposed and adopted the following treatment with perfect success: the limb was enveloped from the toes to the knee in fine cotton wool; well-moulded pasteboard splints were applied on each side; a uniform compression bandage was adopted, the outside being starched. After three days the case was slit open, the edges pared and reapplied, and in a fortnight the patient was convalescent. The pasteboard splint was replaced with emplastrum elemi spread on leather, and a Churton's bandage over all: no further inconvenience was felt. (*Lancet*, April 29, 1876.)

Action of the Coca Leaf.—Mr. Dowdeswell has recently made some observations on this drug, which has been brought into notice in consequence of its having been supposed to be used by Weston in his pedestrian feats, though he has himself denied the statement. The leaf has been known in Europe since the time of the conquest of Peru by the Spaniards; and the earliest account given of it was by Dr. Monardes in 1569. The plant is exclusively cultivated at the present day on the slopes of the Andes, and is a bushy shrub from six to eight feet high when cultivated. The leaves are gathered with great care, and dried. When dried they are chewed by the natives of the country, and are used both to sustain strength and to appease hunger in the absence of other food, and as a stimulant or narcotic in the same manner that opium, tobacco, and alcohol, are used in other countries. A little lime is added to the leaves. Somewhat less than an ounce is chewed per diem. All accounts agree in attributing great powers of endurance to its use, miners, for instance, chewing it every three hours, being able with a handful of maize to work for twelve hours in the mines, while an Indian runner will carry a load of a hundredweight for ten leagues over rough roads in eight hours by its use alone. Stevenson again asserts that the natives, if supplied with it, require no other food for several days whilst performing work. It appears to be doubtful whether it exerts any deleterious influence on the economy or not.

Glycerole of Subacetate of Lead in the Treatment of Chronic Eczema.—Mr. Balmanno Squire gives the following as the best method of making the above preparation. Take of acetate of lead 5 parts, of litharge 3½ parts, and of glycerine 20 parts. Heat for half an hour in a boiling glycerine bath, constantly stirring and filter in a gas oven. The result is a perfectly clear and colourless liquid, which can be readily applied to the skin. This he maintains is perfectly effective in chronic eczema. The first effect of the application, however, is to render the eczematous skin redder and more inflamed as well as moister. The redness he thinks is due simply to greatly increased transparency of the cuticle and of the thin scabs or scales consequent on their infiltration with glycerine, which thus enables the inflamed true skin to shine through them more vividly. The skin, previous to each fresh application of the glycerole, ought to be boldly washed with a soft sponge well moistened with warm soap and water. The strength of the preparation can be readily varied by the addition of a little glycerine. (*Medical Times and Gazette*, March 18th and 25th, 1876.)

Prevention of Spread of Gonorrhœal Ophthalmia.—Dr. Bell Taylor observed that when one eye is affected with gonorrhœal ophthalmia, the other one is very liable to be affected. He recommends, therefore, as a readily accessible and very effective means of preventing the sound eye from being infected, that an ordinary Burgundy pitch plaster should be applied over it with a small hole in the centre, through which the patient is enabled to see, and which may be guarded by a small piece of muslin stuck on the pitch inside. Another method, suggested by Mr. Buller, is to protect the eye by a piece of indiarubber fixed to the brow with a watch-glass in the centre. (*Medical Times and Gazette*, April 1st, 1876.)

Case of Strangulated Inguinal Hernia Reduced by a Novel Method.—The following case, which possesses considerable interest, is reported by Dr. J. Holmes Joy of Tamworth. An old lady at 60 has been ruptured for seven years, and has regularly worn a truss. On April 19th while working in the factory she strained herself, and found that the truss had slipped. A few hours later she was attacked with vomiting and severe pain in the abdomen, when she called to see her medical attendant, Mr. A. M. Sculthorpe, in his surgery. He ordered an aperient and an enema, and desired her to go home to bed. At 2 p.m. (20th) he called to see her, and found the following symptoms present.

She had constant vomiting, and complained of feeling faint and ill. The enema, had been given and had come away accompanied by but little fæcal matter, and the pain was not relieved. The taxis was now tried for upwards of an hour, but with no success; and when he again saw her (about 10 p.m.) the pain and vomiting were present still, and the latter had become stercaraceous. The hernia was hard, as large as a walnut, and renewed and careful efforts to reduce it by the ordinary methods completely failed. He was now consulted as to the propriety of operation, as the symptoms had continued for thirty-six hours, and were becoming urgent. He suggested that, before resorting to an operation, trial should be made of cold applied by the ether spray, and proposed as an adjunct, if this should fail, inflation of the bowels, by means of a common pair of bellows. After raising the patient's hips on two or three pillows, and leaving the shoulder's low, the taxis was tried unsuccessfully. Mr. Sculthorpe now applied the ether spray, which quickly blanched the surface and emptied the part of blood. He now made trial of the taxis, and after some time a small portion of the sac's contents returned, but the greater part was quite immovable. The bellows' point was then well oiled, and introduced per anum, and the application of the spray and taxis was renewed. The bellows (not being double action) required to be withdrawn and filled again. On the fourth bellowsful being pumped slowly in, the bowels were much distended, and by careful pressure and manipulation the hernia was reduced. It was of the oblique variety, but had become (apparently) direct from age; and the size and hardness of the sac-contents rendered their return by pressure on the outer side unaided by the traction from within impossible. This traction was supplied by the inflation of the bowels, while at the same time the hernia was reduced in size by the extreme cold, which the ether spray produced, depriving the parts of blood. The idea of the ether spray for such a purpose is probably not new, and would naturally suggest itself to anyone. Inflation is, I believe, often used in France, and, several years ago, I remember reading of a case in which it was had recourse to in this country, but I have never seen the combination of the two, and in the case I have detailed he believes that ether by itself would not have proved successful. This belief induces him to publish this brief notice. (*British Medical Journal,* May 13th, 1876.)

Extracts from British and Foreign Journals.

The Treatment of Syphilitic Buboes by the Parenchymatous Injection of Iodide of Potassium.—Dr. Jacubowitz, of Nagy-Karoly, starts from the principle that no inflammation to which a degenerative action is attributable is occasioned by the injections but that by this means a solvent of a non-irritating character is brought into direct contact with the glandular tissue. He avoids tincture of iodine, all alcoholic fluids and carbolic acid, and uses instead a weak solution of iodide of potassium in the proportion of about one part to thirty of water. He gives two cases in which he obtained extraordinarily successful results. In one case he made a puncture into the most prominent part of a gland which was enlarged to the size of a goose's egg, pushing the needle in obliquely to a considerable distance. After injecting about the fourth of the syringeful a resistance was felt; he then withdrew the needle for a short distance, penetrated a septum on one side, and again injected a quarter part. By repeating this process, he threw in about fifteen grains of the iodide in an ounce of water. The tumour almost immediately became harder, smaller, and less painful. After four injections performed in the course of two days the tumour gradually dwindled to the size of a hazel-nut, and ultimately vanished altogether. The second case was very similar. Here, however, two dark-blue bodies remained, which were so hard that it seemed to be impossible to inject them. Dr. Jacubowitz however injected hypodermically the iodide on two occasions and with perfect success. Ten injections were required altogether. The small quantity of the iodide required to produce the effects observed is very remarkable. (*Der praktische Arzt*, No. 7, Jahrgang xvi.)

Discovery of Dr. Letamendi in Regard to the Production of Local Anæsthesia.—In a paper on the subject of local anæsthesia S. Cardenal observes, that great difficulties are sometimes experienced in producing local anæsthesia even when the best apparatuses are used and a considerable quantity of ether employed. In endeavouring to overcome these difficulties and to determine the precise moment when anæsthesia is produced, M. Letamendi studied all the circumstances that accompany the production of the anæsthesia, and has established an exceedingly interesting physiological fact. When Richardson's apparatus is used filled with perfectly pure, neutral rectified ether for producing local anæsthesia in any part of the body, at a distance of three or four inches from the skin, the part which is subjected to the spray becomes in the course of two or three minutes strongly reddened and the seat of a disagreeable sensation of cold, but there is never any feeling of burning. If at this moment a slight incision, implicating only the epidermis, and the superficial capillary network of the dermis, be made in the centre of the reddened part, with a convex bistoury, it will be found that immediately after the incision is made, starting from this cut, an anæmic zone of the skin is suddenly produced and continues to enlarge. If now the process of irrigation with ether is continued for some seconds over the part that has become anæmic this region is rendered completely exsanguine, and at the same time perfectly anæsthetic. The ischæmic and anæsthetic tissues resemble butter on section, having lost their suppleness and elasticity. Around the white circle is a zone, the anæmic condition of which is not so perfect, and which resembles a sort of penumbra, though it is strongly disposed to become as perfectly anæmic, as the central area. It is sufficient in fact to direct the ether spray on this zone, to see the anæmic condition propagated to it, and the anæsthesia to become perfect. By this means the anæmic region can be extended in every direction, and can be made to pass round an arm, to ascend as high as the shoulder, or to descend to the forearm. If the irrigation be suspended the effects of course rapidly disappear though the vessels still remain so far affected, that if after the ischæmia has disappeared for some seconds, a fresh jet of ether spray be directed not only on the part originally rendered anæsthetic but anywhere around it an anæmic area can be produced, without practising a new incision. Anæsthetic ischæmia cannot be produced in parts covered with hair, and such parts should consequently always be shaved. Prof. Letamendi is of opinion that local anæsthesia consists in extreme and absolute spasm of the vasomotor nervous system, and that the ischæmia is the result of the complete closure of the vessels. The first sensible effect of the cold produced by the ether on the tissues is, he thinks, relaxation, and consequently vascular dilatation which continues to augment under the influence of the permanent jet of ether. But in order to obtain anæsthesia a movement of reaction must be produced in the dilated capillaries, a movement which will transform their state of exaggerated dilatation into one of extreme constriction; this last effect may sometimes be obtained by prolonging the action of the spray, but in the greater number of instances the vessels remain in the state of extreme exhaustion, atony and relaxation which accompanies hyperæmia. M. Letamendi compares this semi-paralytic state of the dilated vessels gorged with blood to the transitory state of paralysis of the bladder, which results from its over-distention, and in which the distended bladder is unable to expel the urine it contains. It is well known that in this case it is often sufficient to extract a few drops of fluid from the bladder to enable the organ, rendered temporarily powerless, to regain its normal contractility. A slight depletion of blood from the congested vessels, or perhaps a feeble increase of the nervous tension of the capillary network, both of which may be caused by the incision of the hyperæmic integuments may induce the immediate contraction of the vascular system. M. Brown-Séquard states that he has repeated these experiments on two young men, one of dark and the other of fair complexion, and that he is able to corroborate in a very remarkable manner the observations of M. Letamendi, in the man of fair complexion, the incision in the skin required to produce the above-mentioned results was so slight that it was difficult to see it with the naked eye. In both cases the anæstheticised part of the area was hard and white, it had the consistence of congealed fat and an impression was left when firm pressure was made upon it by the finger. (*Archives de Physiologie*, No. 5, 1875.)

Treatment of Nævus.—Dr. John Duncan, in the *Edinburgh Monthly Journal*, states in an article "On the Treatment of Nævus," that he believes with a few exceptions every spreading nævus should be attacked. The means at our disposal may be divided into two classes: first, those by which a scar is avoided; and secondly, those which necessarily leave a cicatrix of greater or less magnitude. The first class aim at the destruction of the texture of the nævus, or the coagulation of the blood which it contains, by agents which can be introduced through minute punctures of the skin. The means used are the injection of coagulating fluids, the introduction of setons, the subcutaneous ligature, electrolysis, and the galvanic cautery. Electrolysis, the other method referred to, may be performed in two ways, viz., with and without insulation of the electrodes. When the needles are not insulated, the operation consists simply in causing more or less complete sloughing of the nævus. The sloughs are precisely similar to those made by caustic acids, if the positive pole be used mainly or alone; to those made by caustic alkalies, if, as is better, the use of the negative predominate. This method, therefore, is one that leaves a scar, and offers no advantage over destruction of the growth by caustic, except, perhaps, in being somewhat less painful; no advantage over ablation, except that the scar is somewhat smaller. Dr. Duncan has used the method, when the skin was largely involved, by passing fine needles under and parallel to the skin. It occasionally does good, especially when combined with other measures; but really no treatment, except entire destruction of the skin, is thoroughly reliable in these cutaneous stains. Electrolysis with uninsulated needles has a very limited application. Electrolysis by means of insulated needles might be regarded as our best method of curing nævi, but that it is more tedious than injection, and less certain than ligature. The needle is insulated by means of vulcanite. The exposed point is made to correspond to the size of the nævus, and care is taken to bury it completely in the growth. The strength of the current may vary between three and twelve cells of Weiss's Smee's battery, according to the distance between the poles, and whether or not anæsthetics be used; and the duration of the action is determined by the feeling of hardness produced. Both poles should be inserted because the current is then more effective, *i.e.*, the moist resistance is diminished, and there are two points of action. The number of needles will vary with the size of the tumour. Dr. Duncan's conclusions may be thus summarised:—First, that in a large number of operations he has never met with an untoward result; secondly, that with moderate care sloughing can be avoided, and suppuration ought never to be seen; thirdly, that with perseverance a cure may always be effected; but lastly, that he has always found electrolysis by this method extremely tedious and troublesome. (*Edinburgh Monthly Journal,* Feb. 1876.)

The Sequelæ and Prophylaxis of Simple Tonsillar Hypertrophy in Children.—Dr. Beverley Robinson remarks that chronic enlargement of the tonsils is so common an affection, that the importance of disturbed function of these glands is apt to be overlooked. In preference to pursuing any active treatment which involves trouble and pain, both healthy and sickly children go on from year to year, bearers of this tumefaction of the fauces without serious attention being directed towards it. Suddenly, however, some acute trouble of the throat shows itself, either idiopathic in its nature or allied with one of the exanthemata, and then anxiety is at once excited; and this frequent condition of additional risk is considered with a greater or less degree of alarm. When enlarged and diseased, the natural function of the tonsils as lymphatic organs is impaired or reduced to zero. Their secretion is altered, becoming cloudy, fœtid, and when swallowed with the alimentary bolus a source of dyspeptic trouble; deglutition is rendered painful and fatiguing, partial obstruction of the posterior nares and compression of the Eustachian orifices is liable to occur, producing difficult respiration and imperfect hearing and vocal intonation. In the prophylactic treatment of chronic enlargement of the tonsils in children, Dr. Robinson attaches great importance to the habit of *cold* bathing. The feet should be kept warm and dry, and he thinks the following rules should be impressed on all mothers and nurses in charge of children:—1st. Not to keep the nursery at a too high temperature, and always to have a basin, pitcher, or bucketful of fresh, pure water with the surface exposed to the air contained in the room. 2nd. Not to allow children to approach a fireplace or register, immediately upon entering or leaving a house. 3rd. Not to keep extra wraps on their little charges when at home, a moment longer than is absolutely essential. Practical attention to the preceding rules, and also to cold bathing, care of the feet, &c., as prophylactic measures of hypertonsillar growth, will be found of unquestionable utility. (*The American Journal of Obstetrics,* No. 3, vol. viii.)

Dental Hygiene.—Dr. Chase considers that dental decay is due to a great extent to the employment of fine wheaten flour instead of unbolted flour as an article of food. The bran and shorts contain a considerable quantity of phosphates while the fine flour contains hardly any, and consequently persons living to any extent upon it do not get the proper amount of mineral salts necessary for the teeth. Especially is this the case during pregnancy, when the mother requires a large quantity of salts for the formation of the bones and teeth of her child as well as for maintaining her own in health. The dental tissues of the fœtus suffer when the mother receives an insufficient supply of salts, and therefore she ought to choose a diet rich in phosphates. In order thoroughly to digest and benefit by it, she ought to pay careful attention to hygienic measures, such as bathing, exercise, pure air, sunlight, and clothing. (Reprint from *Trans. of American Dental Association,* 1866.).

Chlorate of Potash in Vaso-Paralytic Diarrhœa.—By this term Dr. Bonfigli designates the diarrhœa which attends the latter stages of diseases of the nervous system. This diarrhœa is characterized by frequent alvine and serous evacuations, without abdominal tenderness or other digestive disorder, and the autopsy does not reveal the slightest trace of anatomical alteration. The author reports forty-five observations, from which it results that chlorate of potash is one of the most useful remedies in this kind of diarrhœa. He insists, however, that the remedy should be continued a certain length of time, and that it is necessary to give it in doses of 30-150 grs. in twenty-four hours. In cases of cachexia combined with much nervous depression, the remedy acts slowly, and is ineffectual when there is any active disturbance of the intestinal mucous membrane. (*New York Med. Journ.*, from *Gaz. Méd. Prov. Ven.*, Feb., p. 210.)

On Cardiac Hæmoptysis.—Dr. G. Sée thinks that these hæmoptyses, which are sometimes the only guide to the discovery of a cardiac affection, may be determined by a stasis of the blood in the pulmonary circulation, followed by an increase of the venous tension and a rupture of the capillaries. M. Duguet, on the contrary, holds that cardiac hæmoptysis is attributable to the occurrence of emboli in the right ventricle, and especially the right auricle. An important element is the alteration of the vascular walls; so also an increase of the tension in the pulmonary artery; and the author refers to Dr. Barthelmy's remark that cardiac hæmoptysis is frequent, especially in mitral stenosis complicated with hypertrophy of the right ventricle. Hæmoptysis most often occurs in connection with mitral, very seldom with aortic lesions. The characters of cardiac hæmoptysis are—in the majority of cases there is a palpable cause, a violent effort, or more or less prolonged muscular exercise, alcoholic excesses, sudden variations of temperature, pregnancy. In general, the hæmorrhage is slight, and the blood is usually of a dark-red, sometimes black colour. The expectoration may become pneumonic, especially if inflammation has taken place around the infarction; sometimes it has a garlic-odour; it is often thick, viscid, and adheres to the vessel. When there is hæmorrhage, though examination of the heart and lungs reveals nothing abnormal in these organs, the case may be one of hysterical hæmatemesis. (*New York Med. Journ.*, from *Lyon Médicale.*)

Abortive Treatment of Erysipelas by Quinine and Opium.—Dr. Le Roy Satterlee recommends the following treatment, to which he has given the name of the quinine and opium treatment for erysipelas. It consists in the administration of one, two, or three full doses of the sulphate of quinine combined with enough tincture or elixir of opium to moderate the disagreeable effects of the quinine upon the head, and to assist sleep. If called at the beginning of an attack of erysipelas, he administers, to an adult, twenty-five to thirty grains of the sulphate of quinine, dissolved in one and a half ounces of water, which is readily accomplished by the addition of a little dilute sulphuric acid; a few drops will completely dissolve the powder and a clear solution will be formed: to this he adds fifteen minims of McMunn's elixir of opium, and thus forms a draught which, although very bitter to the taste, is not so disagreeable to take as a small powder of quinine; in fact, he on one occasion administered sixty grains of quinine, dissolved in three ounces of water, in one dose, to a patient with a very obstinate and long-standing intermittent fever, and the remark he made some time afterward was that he "was so glad that that draught had been given him *instead of quinine*, as he had taken a great many quinine-powders for over two years, and they were very unpleasant to take, without doing him much good." Having ordered a draught, as just stated, containing twenty-five or thirty grains of the sulphate of quinine, he directs the erysipelas patient to take it all at once on retiring for the night. It will usually be retained by the stomach without difficulty; if, however, the stomach is irritable, he prescribes a mustard-plaster about the size of the hand, to be applied ten or fifteen minutes before taking the dose, under the left breast; this procedure he has found unfailing in quieting the stomach so that the draught is retained. In one case, where the fauces were greatly inflamed and deglutition very painful: an equally good effect was produced by administering the dose by the rectum. After this draught the patient usually has a very good night, sleeping well and perspiring freely; and, on examination at the end of twenty-four hours, the temperature and pulse are found to have fallen greatly, the general symptoms have either disappeared or been very much improved. In some cases deafness and noise in the head from the quinine are experienced, but in the majority of instances the opium seems to entirely remove this after-effect of the drug. The eruption markedly diminishes, and he has seen many cases where a single draught has completely aborted the disease. In all cases he directs the patient to observe simple hygienic rules, use a stimulating diet, with free draughts of lemonade where there is biliousness, a simple cathartic in cases of constipation, and no external application whatever. This is his treatment in the incipiency of a mild attack of erysipelas. But in any and all of the varieties and severer forms of the disease, or where he does not see the case until it has advanced several days, he commences treatment in the same manner, but, at the end of twenty-four hours, or on the second evening of his attendance, he administers a second quinine draught, and, if necessary, a third at the end of forty-eight hours. In his experience this has been entirely successful in the most severe types of the disease, the eruption and general symptoms passing away with rapidity. The patient makes an excellent recovery under this mode of treatment, the appetite comes speedily, and there is very little debility experienced. Twenty-four or at most forty-eight hours is all that is required to abort the disease by this treatment. Having used it for three years in a large number of cases, he has never found any disagreeable after-effects; on the contrary, the general health of the patient is improved, and this is the experience of all those, whom he has known, who have employed this plan. (*New York Med. Jour.*, Dec. 1874.)

Uræmic Convulsions Treated by Morphia.—Dr. Fred. E. Potter, records the following case:—Mr. D., aged 62, for the past year has been suffering from Bright's Disease. On the 31st of January, 1874, he was seized with uræmic convulsions, each fit lasting from five to ten minutes, with from ten to twenty minutes of consciousness intervening; the consciousness during the intervals gradually decreased, until he lay between the attacks in a deep stupor, breathing stertorously. As all means tried for relief proved unavailing, and as the patient was evidently failing rapidly, Dr. Potter injected into the cellular tissue of the fore-arm, in the midst of a convulsion, twelve minims of Magendie's solution of morphia. For four hours following, the convulsions did not recur; he then had two light attacks, after which there was no return. In ten days he had so far recovered as to be dressed; he had partial paralysis of the right side and hemiopia, the left half of each retina being impaired. During the past year the patient has had no return of convulsions; he complains of muscular weakness in the right upper extremity, with sensation of numbness of the right half of the body. The hemiopia has but slightly improved. The result in this case, Dr. Potter thinks, goes far to show that the danger to be apprehended from the use of morphia in uræmic convulsions has been much exaggerated. The effects of opium upon the cerebral circulation whereby the comatose condition arising from a poisonous dose of that medicine is produced, in all probability differ widely from those which are caused by the non-elimination of the effete matter which in Bright's Disease gives rise to uræmic convulsions; and though there is a strong analogy between the effects of the drug and of the poison, the above case tends to show the groundlessness of the fear that by administering opium we shall add fuel to the fire. (*Bost. Med. and Surg. Journ.* No. xxi., vol. xvii.)

The Pathology of Sunstroke.—Prof. Arndt, of Greifswald, observes that in the last few days of the month of July, 1870, the first division of the Second Army Corps made a march from Berlin. The rendezvous was at 10 A.M. in the open country; several were already faint, and the order was given to march in open column, to open the neckcloths and the top buttons of the coat, and to exchange the helmet for the cap. After the rendezvous, in which the soldiers had a very insufficient supply of drink, they marched slowly to Bankow and Schönhausen, but many fainted. After marching for half an hour cases of true sunstroke occurred; many men fell down as though dead, or turned round and round, or moved here and there as though affected with violent cramps. Bankow was marched through in order to avoid the effects of drinking too much water. Of the more severely affected seven died; and Prof. Arndt had the opportunity of making a post-mortem upon three, which all presented the following similar characters. Two points which are somewhat opposed to one another were first noticed; the paleness of all the organs, and the repletion of all the larger vessels with dark coloured uncoagulated blood; the skin and the muscles were themselves pale, but the larger vessels were greatly distended; the brain and its membranes were anæmic, but the sinuses and large veins were excessively full of dark uncoagulated blood. The same relation was observed to exist between the substance of the heart and the coronary vessels, the liver, kidneys, and the mucous membranes of the bladder and of the intestinal canal. The engorgement of the larger vessels was so great as to lead to rupture and ecchymoses of the vessels, under the pericardium and endocardium, under the pleuræ and in the pelvis of the kidneys. In spite of this repletion of all the large vessels the interstitial tissues of the organs exhibited great poverty or entire want of blood; there was in fact parenchymatous anæmia: this parenchymatous anæmia resulted from œdematous transudation. The brain, liver, and kidneys were swollen; the brain tightly fitted the sac of the dura mater; the convolutions of the hemispheres were broad, flattened and compressed, so that the sulci were almost obliterated; their substance was always moist, sections appearing as though wetted with water; the ventricles contained much serum. The membranes, which were in part swollen like a bladder, were easily separated from the brain; the liver and kidneys presented the same appearances. It thus appeared that the serous transudation into the parenchyma caused compression of the smallest vessels and capillaries, driving the blood into the yielding veins. The heart, liver, and kidneys exhibited other peculiarities; the heart was hard, its tissue was discoloured, pale dirty red; it was contracted, dry, and friable, and the cut surfaces, especially towards their thin edges, were glassy. The same characters were presented by the muscles of the whole body; the liver appeared as if it had been boiled. The cortical substance of the kidneys was expanded and appeared swollen. In these organs therefore there was not only œdema but a change of the elements, a kind of cloudy swelling which he regards as the forerunner of parenchymatous inflammation. Similar changes are observed in the principal organs of the body in patients who have died from typhus, small-pox, and pneumonia, if the disease prove fatal at its height and in consequence of the high temperature. Sunstroke only occurs in our climate on very hot days, and in those who have severe work to do as in the case of marching troops. Three degrees may be distinguished. First degree: the temperature rises from 38° to 40° C., the pulse is 100° to 120°, the respiration is 30 per min. or more; perspiration and thirst are experienced, the head is affected, and the patient suffers from muscæ before the eyes, noises in the ears, faintness, and tightness of the chest; he is irritable and taciturn. Rapid and complete recovery results from the application of cold. Second degree: if no notice be taken of these premonitory symptoms, if no rest or water be obtained, all the symptoms increase in intensity, the temperature rises to 41° or 42° C., the pulse is 130 to 140, the respiration 40 irregular; perspiration abundant, thirst unbearable; all objects seem to vacillate before the eyes, and the field of vision becomes dark, the noises in the ears like the striking of bells, humming and buzzing sounds are experienced, tightness of the chest increases to deadly anxiety, the colour changes, becoming pale and bluish or livid; though they feel excessively hot the pulse can hardly be felt, consciousness is lost; if a vein be opened, only a little and remarkably dark blood flows. The tongue is coated and there is œdema of the face, hands, and feet. In the third degree the temperature rises to 44° C. The skin is no longer moist, but dry and burning. Respiration is difficult. There is well-marked anxiety about the præcordial region. The patient suddenly collapses and lies immovable, or rolls over in convulsions. The fæces escape involuntarily. The face is expressionless; the eyes are immovable; the lips and tips of the fingers bluish; the pulse imperceptible; venesection only yields a teaspoonful of dark, red, uncoagulable blood. In temperate climates the case rarely terminates in an attack of fulminating apoplexy. The greater number partially recover, and then fall in the course of a few hours into a state of coma, in which they die. Others remain through life epileptic, or live with their mental powers permanently impaired and with more or less disposition to insanity. The alteration of the condition of the blood has been regarded by some as the cause of the symptoms observed during life. Obermeier found the thin fluid, non-coagulating, dark-red blood possessed an acid reaction, and was richer than natural in urea and white corpuscles. Many believe the overcharging of the blood with carbonic acid to be the cause of the symptoms. Obermeier also observed an excess of lactic acid and of carbonate of ammonia in the blood, and it certainly contains an excess of the products of regressive metamorphosis. M. Arndt attributes the symptoms to the degree of cloudy swelling of the several organs. The fulminating cases he thinks are due to myocarditis; the subsequent symptoms to a parenchymatous inflammation of the brain, similar to the conditions of disease associated with very high temperatures of the body in infectious diseases. (*Der Prak. Arzt.*, No. ix., 1875; from *Virchow's Archiv.* B. lxiv. 1.)

Notes and Queries.

NOTES AND CORRESPONDENCE.

Cod-liver Oil Jelly.—We have received from Mr. Pratt, of York, a specimen of Agnew's cod-liver oil jelly. It consists of cod-liver oil 72 per cent., white sugar 16·8, citric acid 0·6, gelatin 2·7, water, 7·5, and essential oil 0·2 per cent. It therefore contains nearly three-quarters of its bulk of pure cod-liver oil, the taste of which is admirably disguised by the gelatin and essential oil with which it is mixed. We have not had it analyzed, but so far as palatability is concerned we think it one of the best preparations of cod-liver oil yet submitted to our notice; and we recommend those who wish to give cod-liver oil either to adult patients who dislike it, or to children, to give this jelly a trial.

Morphia and Atropia in Delirium Tremens.—Surgeon-Major Lougheed writes, that he has been induced by the perusal of Dr. Oliver's paper on the subcutaneous injection of morphia and atropia in the February number of the *Practitioner*, to try it in delirium tremens, cancer, carbuncle, burns, and acute rheumatism. In all these diseases he has found it most useful. In a case of delirium tremens one injection of $\frac{1}{8}$th of a grain of morphia, and $\frac{1}{120}$th of a grain of atropia arrested sickness and procured sleep, although three times the dose of morphia alone had been previously employed unsuccessfully.

Turtle Soup and Turtle Jelly for Invalids.—Messrs. Brand have sent us specimens of their turtle soup and turtle jelly, prepared from freshly killed turtles, and intended either to sustain the strength of invalids during acute disease or for their use during convalescence. Both preparations appear to us to be excellent. When a little sherry is added to the soup it acquires the rich flavour so well known at public dinners; but it is possible that many invalids may prefer it without this addition. Both soup and jelly are likely to be useful as a substitute for, or an addition to ordinary beef tea and jellies in cases where patients are becoming tired of them or dislike their flavour.

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⁂ Any of the foreign works may be procured on application to Messrs. DULAU, of Soho Square, W.C.;—WILLIAMS and NORGATE, of Henrietta Street, Covent Garden, W.C.; or BAILLIERE, of King William Street, Charing Cross.

Department of Public Health.

ON INFECTIOUS DISEASES.[1]

BY S. STUCKER.

As an introduction to my paper I may be permitted first of all to discuss the current signification of the expressions "contagion" and "miasm."

I believe my object will be most readily attained if I make use of two expressions suggested by Pettenkofer. Pettenkofer proposes the term *entogen* for an agent of disease which can undergo increase in the affected organism; and on the other hand, the term *ectogen* for one which reproduces itself outside the body. Using these expressions, it may be said that contagions are "entogens," and miasms are "ectogens."

If we add that the capacity of being carried in an active condition, or rather in a condition capable of becoming active from one organism to another, is possessed by contagia but not by miasmata, we shall include all that is usually understood by these terms.

In the case of the greater number of diseases which are regarded as originating in contagia or miasmata, the real agents are indeed unknown. But inasmuch as some of the peculiarities of such causes may be deduced from their mode of action, these characteristics are employed for the purposes of classification, and the terms contagion and miasm are merely the symbols employed to indicate corresponding groups.

One of the principal characteristics of these groups is the capacity of the agents of disease to multiply either within or without the organism. If, for example, we see syphilis propagating itself from individual to individual, so that successive generations become inoculated, we are compelled to admit a cause of syphilis which undergoes increase in syphilitic patients. If, on the other hand, we observe that malaria, as a rule, is limited to certain districts and either continuously or periodically affects a larger or smaller part of the population, we can only explain the phenomenon by admitting that the cause of malaria is generated in these regions.

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[1] Read before the "Mittheilungun des Verrein der Aerzte in Nieder-Oesterreich." Band ii. No. 4.

A second characteristic which may be deduced from the effects of the agents of disease is, that they must be material *substances* which must penetrate into the organism in order to cause the disease.

Every action on our earth undoubtedly takes place from substances, but it is not every action that necessarily causes a movement of the mass of the body. Heat, light, and electricity, may act on our organism and become causes of disease without occasioning any movement of the whole. Apart, however, from the many and to some extent successful efforts that have been made to demonstrate the existence of agents of disease capable of undergoing multiplication, the typical course of the diseases in question leads us to the conviction that we are dealing with some bodies capable of being introduced into the organism.

The course of these diseases, in comparison with the rapidity with which the so-called unponderable agents propagate themselves, is extremely slow. A man is not immediately affected by malaria if he enters a malarial region even when he is predisposed to the disease. He is not at once attacked by cholera, even granting again that, being predisposed to it, he goes into a focus of cholera; and the same is true of all similar affections. A much longer time must elapse from the moment of exposure to the causes of disease, until the disease can be actually recognised; sometimes several hours, generally several days, and occasionally several weeks, elapsing. We even know that the disease may break out after the patient has long left the place where he has been exposed to it, and no doubt can be entertained that in such cases the germ of the disease has been received in a certain place, whilst the disease itself has broken out after the lapse of some days or weeks in another. Such phenomena are not in accordance with the view that we are here *only* dealing with the propagation of forces, or perhaps of ether in the substance without molecular movement.

We may consequently regard contagia and miasmata as *substances*, where they have even not been directly obtained in an isolated form, which must come into contact with the organism in some way or other, if they are to affect it.

So far as the introduction of certain injurious substances into our bodies can be described as an infection of them, the diseases by which they are produced are termed infectious diseases; and in so far as the capacity of multiplying themselves is attributed to certain infecting substances, they may be compared with the substances that excite fermentation. The diseases themselves are also compared to fermentation, and they are therefore called "zymotic diseases."

Although the scope of this address does not permit me to discuss this subject at large, I shall still avail myself of the expressions "infectious material," and "zymotic material," on account of their convenience, since we may employ each of these words as both a collective term for contagia and miasmata, and also as expressing each of them separately.

I have already mentioned that contagia are regarded as communicable; miasmata, on the other hand, as non-communicable. Pettenkofer has enlarged this conception by subdividing the non-communicable ectogenous substances into transportable and non-transportable. He was led to do this by the consideration that an infectious material in quantity sufficient to exert its effects is capable of adhering to the surface of a body, of being carried about with it, and of penetrating at some distant place into the body of a man and of making him ill. If this material accidentally adhere to a patient in whom it has already produced disease, the disease may pass from one man to another; and one may thus be induced to associate it with the presence of a real contagion. Thus it occurs, he says, that cholera and abdominal typhus are regarded as contagious, whilst they really have only been communicated by transport of the cause of disease. As the formal side of this view cannot be attacked logically, we must accept it as one of the fundamental divisions, and consequently the provisional arrangement of infectious materials may be given as follows:—

1. Entogenous and communicable (*übertragbar*) . Contagia.
2. Ectogen and transportable (*verschleppbar*) . } Miasmata.
3. Ectogen and non-transportable . . . . . }

Further consideration shows us, however, that even this arrangement is not perfectly satisfactory. In the first place it is quietly assumed that all entogenous infectious materials are communicable, which has nevertheless not been in the slightest degree proved.

Liebermeister has lately advanced an hypothesis according to which entogenesis may be present, and communicability may even be excluded. According to this hypothesis, the agent which produces typhoid fever may multiply in the organism, but the fresh germs thus produced are inoperative so long as they remain within it. In order that they should become operative they must first pass through a second phase of development outside the organism. The chief ground for this hypothesis is the fact that it appears questionable whether the recent dejections of typhoid patients contain the poison in an active condition, physicians and nurses being rarely affected, however much they may have to do with the fresh dejecta. Other patients lying in the same room with those suffering from typhoid fever are likewise rarely attacked, although it is undoubtedly demonstrated by facts that typhoid can be spread by means of the patients' stools.

This is no doubt one mode of explaining the phenomena, though I do not believe it will be accepted by physicians. In the first place, the hypothesis presupposes the contested point of the non-contagiousness of typhoid fever as completely and necessarily demonstrated; but this is by no means the case. The utmost that anyone can say is, that he has never been able to establish a single case of contagion. It may further be added that those cases in which other observers believe that they have met with contagia are perhaps founded on error, and perhaps are referable to transportation of the disease.

But such statements and opinions do not together possess the weight of an actual proof, for they do not take into consideration the well-known fact which Ranke has recently again dwelt upon, that epidemics vary in their character. Secondly, the hypothesis is superfluous. If we admit that typhoid fever spreads by means of the dejecta, as is represented by Murchison, Gietl and others, the simple explanation advanced by Bierner, that the poison of typhoid fever increases in the intestine and is discharged with the dejections, is quite sufficient. This explanation leaves the contagiousness of the disease an open question. An agent that increases in the intestine is not necessarily excreted by the lungs or skin, nor can it be excreted in such quantity as to be capable of transmission in an active condition in the ordinary course of human intercourse. This explanation does not presuppose definitely that the dejections in the recent state contain sufficient virus in order to pass through the air in quantity capable of exerting its action, for we do not know how long the freshly secreted intestinal juice remains in the intestine before it is excreted, and how much time is consequently afforded for the introduced germ to increase in it.

I, for my part, am very much indisposed to take any part in the controversy regarding the contagiousness or non-contagiousness of typhoid fever. But since the course of my observations leads me to advocate the entogenesis of the virus in a series of diseases to which typhoid belongs, I have considered it my duty first of all to show which of the ordinary views regarding typhoid allows us to admit entogenesis without prejudicating the decision of the question of the contagiousness of the disease. In now entering upon this discussion, I must first state my views upon the value of the word communicable (*übertragbar*). Whether any infective disease is absolutely non-communicable has not as yet been determined by anyone. The most we can say is, that a definite disease, so far as our knowledge goes, has or has not been found to be communicable, either by ordinary human intercourse or by appropriate experiments. This expression may be employed, for example, in regard to the malarial diseases so far as they occur in Germany. Whether malaria cannot be communicated under any circumstances it is impossible to say. Granting that malaria originates in consequence of the entrance of some substances into the organism—and of this no one now entertains any doubt—the possibility of communicating these substances in an active condition from one person to another by some definite procedure is at least conceivable.

If, then, we designate the miasmata as non-communicable, this expression by no means logically excludes the possibility of their being communicated; nor do we say that this never under any condition takes place, or can take place, but we merely apply a rule that has been obtained from practice. The same holds good of the expression that the malarial agent is not transportable (*verschleppbar*). We again only signify by this, that experience has shown that it is not transported in the ordinary mode, like the cholera poison. At the same time, we know that it can be transported for short distances in an active condition by the wind.

This being the state of things, and the extracts I have already given from Bierner being borne in mind, no one can well hold the opinion that entogenesis of the infective material is excluded in the case of a zymotic disease, which is non-communicable in the ordinary sense. Further consideration teaches us even that it is not only not excluded, but is not improbable.

I shall introduce this consideration under the supposition that we are here only treating of chemical actions. It sometimes happens indeed that low organisms penetrate into the human body, and there grow as from a focus so abundantly, that they displace the tissues, occlude the vessels, and in short become injurious owing to mechanical disturbances. The affections arising in this manner are rightly regarded as zymotic, but in these cases evidence of internal multiplication of zymotic substances is not required.

If we only regard the chemical action of the excitors of disease, we may designate those which are not "engenetic," as poisons in the narrower sense of the term, since a certain dose of them large enough to operate must enter the organism at once, as is the case with strychnia, potassium cyanide, and other poisons. In whatsoever condition of aggregation such a chemically active substance may be introduced into the body, whether gaseous, solid, or fluid, it must, in order to operate, either dissolve in our bodies, or these substances must be in themselves organisms. In regard to chemical actions, the proposition "*corpora non agunt nisi sint soluta*" has only one exception in "*corpora animata.*" On the other hand, the capacity of increasing can be ascribed to non-crystalline insoluble bodies only when they possess life. In other words, bodies which, when not in a state of solution, act chemically upon their surroundings, and are capable at the same time of multiplication, are termed organisms.

Thus this consideration also leads us to the interesting result that the question whether chemically acting infective substances are dissolved or suspended is of equal importance with the question whether these substances are alive or not. If we now enter upon the question whether entogenesis is improbable in non-communicable zymotic diseases, we must keep both cases in view, viz., the case of the *miasma vivum* and that of the *miasma solutum.* It appears to me that it is not difficult to answer this question. Entogenesis is by no means improbable in the case of all those forms which exhibit a long period of incubation.

It has already been shown by various means that a long duration of incubation demonstrates the existence of an agent which reproduces itself in the organism, and only acquires its full activity when a certain amount has been developed. I shall, however, give my answer at somewhat greater length.

Although but few poisons act immediately after their introduction into the system, some minutes or several hours elapsing before they exert their influence, yet no poison is at present known which can remain for several weeks in the organism without acting, and then break out with its full violence.

It is, however, conceivable that such poisons may exist, possibly in consequence of their being extremely insoluble, and therefore many days or weeks may pass before a sufficient quantity of them may be dissolved in the animal fluids. There is, however, little probability that such is the case for the above-named miasmata. The miasmata are for the most part taken up during the respiratory acts, entering the organism either in the form of a gas or in an extremely finely-divided condition, if, indeed, they be not small organisms themselves.

In the latter case there is no question in regard to the solubility of the poison, and scarcely anyone will doubt the occurrence of entogenesis. In accordance with all that we know of the lower organisms, they undergo multiplication wherever they exert their chemical action. It is thus by no means in accordance with our experience to admit that small organisms penetrate into our interior, remain there for a long time, and act without undergoing increase.

But if we suppose that the miasmata are not organisms, but gases or finely-divided inanimate particles, it is still less probable that when in sufficient quantity, after their entrance into the organism, they should require several days for the production of their action, especially since they are placed under favourable physical conditions for their solution.

We thus see that entogenesis in the case of the non-communicable miasmatic diseases, in so far as they exhibit a long duration of incubation, is neither impossible nor improbable.

In such a position of things, however, is it permissible to divide infective substances into entogens and ectogens only, or, what is the same thing, into contagion and miasmata?

Hitherto this difficulty has been overcome, and is still got over to some extent by the formation of an intermediate group, to which the term of miasmatic-contagious has been applied.

But it appears that instead of reconciling the opposing parties by the formation of this intermediate group, we only render it more difficult to reconcile them.

Many observers agree that malaria is not contagious-miasmatic, but a pure miasmatic disease; but if there be the slightest probability in the case of any disease, with a long period of incubation, that the virus increases in the interior of the patient, this rises almost to certainty in malaria.

Is it conceivable that a poison which does not increase in the body after it has once acted, and when the organism has come completely to rest, can again rise to its full intensity of action, and repeat this play for weeks or for months?

It might be said that the periodicity is founded upon the human organisation, since we see the animal heat undergoing daily-recurring variations.

I do not quite agree with the statement, but, as I am unable here to enter fully into the question, merely mention it as an argument.

But even thus, the essential ground of the disappearance and reappearance of the action is not in the slightest degree explained. It does not explain why ague sometimes presents a two, and at others, a three or four days type; and it leaves completely unexplained the phenomena of relapses. We know that a man who has left the malarial region, and who goes to a land where no genuine case of malaria has occurred for many years, remains for a long period without an attack, and may then after some disturbance of his health again suffer from periodic attacks.

It will be objected that we simply know nothing about the periodic types and relapses of ague, and that speaking generally we have no knowledge by means of which we can afford an explanation of these phenomena.

This is not quite correct. We are acquainted with two zymotic and febrile forms of disease which pursue their course with decided intermissions; namely, intermittent and relapsing fever. In regard to the active agent of malaria, we know little or nothing. The assumption that the disease is due to gas, upheld largely by Schwalbe, is far from being proved. On the other hand, the statements of Salisbury and his followers, according to which the malarial agent emanates from certain alga cells, is neither plausible nor sufficiently founded. In regard to the relapsing fever there is, on the other hand, a fact which is of considerable importance, I mean the discovery by Obermeyer of certain organisms, termed spirilla, in the blood of those affected with this disease, a discovery which has been since substantiated by Engel in the clinic of Frerichs, and subsequently by Weigert in Breslau.

According to this the spirillæ only appear in the blood at the time of the paroxysm, and disappear at its close.

If these statements be accepted, and there seems no reason for doubting their correctness, since they are supported by so many trustworthy witnesses, the occurrence of a certain phenomenon in the blood, and a paroxysm, can no longer be regarded as a mere coincidence, and we have here an explanation resting on a basis of fact, an explanation which is to the effect that the paroxysm is due to the reproduction, and its termination to the suppression, of a generation of organisms, the germs of which however remain, and render a second paroxysm possible.'

I am far, however, from accepting this explanation. But in regard to the objection, that we are unable to explain with our present knowledge the occurrence of the paroxysms and intermissions, I would observe that it *is* certainly possible to do so, and further, that we are quite helpless in the matter only when we exclude entogenesis.

The society may perhaps advance the objection, that I have imperceptibly passed from the defence of entogenesis into an argument in favour of miasma vivum, and this in those very forms of disease, namely malaria, in which it is most doubted.

I do not belong to these doubters; my private opinion in regard to the malarial agent is very strongly formed. But I am fully aware that private opinions have no claim to possess scientific value. If I nevertheless refer to the possibility of explaining the course of malaria by admitting a living virus, it is only because a discovery of the parallel form, namely of relapsing fever, has led to it; moreover, everyone is at liberty to admit an inanimate ferment instead of living organisms. I have pointed out whilst admitting that malaria is an entogenic virus, first, that this view is not excluded; secondly, that it is not improbable; and thirdly, that it is a mode of explaining the complicated phenomena. For the substance to which I should like to ascribe entogenesis may provisionally be indicated by X, in order that every one may at his will regard it as animated or inanimate.

But as soon as we admit entogenesis of the poison in the case of malaria, we can no longer regard it as purely miasmatic.

We thus see that we are scarcely able to include it in the ordinary scheme.

The establishment of contagious miasmatic diseases, I maintain, renders difficult the reconciliation of the contending parties. It is, for example, a strong statement to say that the virus of typhoid fever is not entogenetic. The opinion which has received general acceptance, that the dejections contain active material, proves the entogenesis of the virus. I have already pointed out that the duration of incubation of typhus is in favour of it, and the protracted course of the disease certainly supports this opinion. But as I have shown, there is not the smallest reason for the opponents of contagiousness to doubt entogenesis from the moment that we are satisfied that entogenous agents are not in the ordinary sense of the word communicable.

If on the other hand we designate the virus of ileotyphus (typhoid fever) as amphigenous, something is gained, for an explanation is obtained of a number of peculiarities which all parties recognise unanimously, whilst it leaves open the contested question of contagiousness.

It appears to me, therefore, that if we wish to divide infective substances in accordance with their place of origin, we shall but accomplish an object by making three groups, namely :—

1. Entogenous, of which syphilis is undoubtedly an example.

2. Amphigenous, as in the case of the agents which have been experimentally found to produce diphtheritis, and lastly,

3. Ectogenous, to which all those poisons belong that produce simple poisoning.

This classification has no reference to the question of communicability. Its principle is quite trustworthy, for whatever advances may subsequently be made in our knowledge, every infective substance must be included in one or other of these groups.

However important and advantageous, on the other hand, it may be to ask in regard to the zymotic diseases, whether they are contagious, and however little I approve of giving up the use of the expression "miasma," I should still strongly oppose the employment of these symbols as group-characters. In opposition to contagiousness, we must, according to the procedure of Pettenkofer, establish the transportability (*verschleppbarkeit*) and non-transplantability (*Nicht-verpflanzbarkeit*). But are the opposites here really illimitable? Is that unquestionably contagious disease syphilis not transportable by surgical instruments, &c.? Is this not also the case with the greater number of ectogenous substances? And lastly, can it be said of any infective substance that it is not under any circumstances communicable?

Griesinger has already directly, and Hirsch, in his *Year-book*, has indirectly demonstrated that it is impossible to include all infectious diseases in contagia and miasmata, and I will only add, that this should not be taken as a ground of division generally.

After these somewhat extended remarks, I may certainly express myself very briefly in regard to the main fact, that is to say in regard to the limits of the group of infectious diseases.

A logical discussion on this point is superfluous, for we are not considering what should properly be understood by the term infective disease, but what is ordinarily meant by it; in regard to the modern use of the word all are agreed.

In the older literature the word "infection" was applied to the introduction of pus into the blood at a time when the zymotic nature of pyæmia was unquestioned. So far as my historical knowledge extends, this expression was first introduced for "pyæmia" by Sedillot, and long subsequently Roser proposed for the first time that pyæmia should be considered a zymotic disease. It hence appears that originally, as we have seen, infection and zymosis were not treated as synonymous terms.

The group of zymotic diseases was first treated by Griesinger under the name of infective diseases, and, if I am not mistaken, it was the great authority of Griesinger and A. Hirsch that effected the introduction of this term into German literature.

In the meanwhile Aug. Hirsch includes the animal poisons without reference to their essential nature in the class of infective diseases. This might be admitted on the ground that these poisons are produced by animals, and are thus reproduceable. But in this sense what is not reproduceable?

If on account of this characteristic we refer the animal poisons to the infectious substances, why should not the vegetable poisons also be referred to them? We have in both cases to deal with poisons that are produced by organisms. What would be said if it were established that an animal produced sulphuric acid in its poison glands, and that its bite was only on that account injurious? And if the sulphuric acid produced by an animal be termed an infectious substance, why should not the sulphuric acid produced in the human body be also called so?

In consequence of this, it will be urged against me—the proposition of A. Hirsch should not be followed, and we should only include, as some recent writers do, amongst the infective diseases those diseases the virus of which is capable of reproducing itself. But what is gained by this? Fancy, if the term ophthalmic disease were hereafter to be limited to the diseases of the cornea, we should be thereby compelled to reiterate in the most impressive manner to the rising generation, and to all those who are not immediately interested, that they must take care not to err, and not to understand by the term ophthalmic disease all that might be included under that term. We are in exactly the same position in regard to infectious diseases. Under the term infection we might best include or represent that which the word actually expresses. But this we must not do, and must only include those diseases under this term which behave like ferments, that is to say, the active agents which are capable of spontaneous multiplication. But for these diseases we have an excellent designation in the expression "zymotic diseases," a term that spares us all the confusion in which some authors have plunged us by endeavouring to accomplish an impossibility, and striving to define and arbitrarily introduced expression.

For example, the infective diseases have been defined as being produced by some unknown material. This definition places the whole group on the retired list, as we must hope that with the progress of knowledge all these substances will cease to be unknown.

I therefore consider it to be advantageous to call those diseases zymotic in regard to which we admit that they, like ferments, are capable of spontaneously multiplying their causes, and to regard infective diseases as a wider class of which the zymotic diseases are only a subdivision. The animal poisons may thus be included amongst the infective diseases without necessarily belonging to the zymotic.

But I shall be asked what then should be the limits of the conception of infective diseases? I am unfortunately unable to reply to this question.

I regard the introduction of *terms*, howsoever distinguished the person from whom they proceed, and however appropriate they may have originally been, as no unmixed good. And I am by no means of opinion that any exposure of the evil that I may make will lead to any material change. The expectant method can here alone be recommended as worthy of adoption, that is to say, the recognition of the inappropriate use of an expression and its silent disuse. The best that can be done under the circumstances it appears to me is to follow the procedure of A. Hirsch, and to include animal poisons amongst the infective diseases, and thus, at least, to foreshadow that the limits really extend somewhat farther, and that yet other toxic diseases may be included, even if they are not already included, under other titles.

RECENT WORKS ON CHOLERA.

The literature of cholera has recently been enriched by two works, both of exceptional value. The one relates to cholera in East Africa, the other to the cholera epidemic of 1873 in the United States.

The first of these works[1] is by Dr. James Christie, of Glasgow, late physician to the Sultan of Zanzibar, and one of the Foreign Secretaries of the Epidemiological Society of London. It is unquestionably the most important contribution to our knowledge of cholera which has appeared for some time. It gives the previously all but unknown history of the several diffusions of cholera which have taken place in East Africa. In 1870 and 1871 Dr. Christie, then resident in Zanzibar, communicated to the Epidemiological Society two papers on cholera in East Africa, which, as our contemporary the *Lancet* at the time remarked, opened out an entirely new chapter in the history of the disease. So little had been ascertained of the diffusions of cholera in East Africa at the time that it was not until the first of the communications referred to was received by the Epidemiological Society in 1870, that it became known that the great epidemic which had spread into Arabia in 1865, and thence extended over Europe and to the New World, had spread also widely into East Africa. This discovery rendered necessary a reconsideration of the opinions which, to that time, had been commonly entertained of the origin of the epidemic referred to, and eventually led to a more accurate study of its source and lines of dissemination. The study of the progress of this epidemic in East Africa by Dr. Christie is probably unique as a piece of historico-geographical research, and affords a remarkable confirmation of the opinion entertained by perhaps every pathologist of note in Europe, that the initial fact of the spread of cholera, beyond the confines of India at least, is the dissemination of the disease by human intercourse. With a rare knowledge of local languages, with an unwearied perseverance which strikes one with amazement when exercised under the depressing influence of the tropics and the Malebolgian horrors of Zanzibar, Dr. Christie pushed his inquiries among natives from the mainland and the leaders of caravans, until he discovered the chronology of the different appearances of cholera, from 1865 to 1871, between the east coast and the Gulf of Aden in one direction, the confines of Abyssinia in another, Central Africa in a third, and the Mozambique in a fourth, quite unaware at the time that he was supplying a missing chapter in the great pandemic diffusion of 1865-71. In this book he has told the story of this inquiry in detail, and of his further researches into preceding diffusions of cholera in East Africa. Treating of countries, localities, and peoples little known, and many of which find no place in modern atlases and geographies, he has given such account of them as serves to make the conditions of the spread and development of cholera in East Africa comprehensible, and his book readable and valuable quite apart from its special subject. To epidemiologists this work will be most welcome, and we commend it to all else who take an interest in cholera or in East Africa. The book is illustrated by two admirably executed and very necessary maps.

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[1] Cholera Epidemics in East Africa: an Account of the several Diffusions of the Disease in that Country from 1821 till 1872, with an Outline of the Geography, Ethnology, and Trade Connections of the Regions through which the Epidemics passed. By James Christie, M.A., M.D., late Physician to H. H. the Sultan of Zanzibar, one of the Foreign Secretaries of the Epidemiological Society of London. Macmillan and Co.

The second work we have referred to [1] is not less interesting to the public-health student and epidemiologist in its way than Dr. Christie's work. This second work is the official account of the Cholera Epidemic of 1873 in the United States. This epidemic was remarkable from its appearance in New Orleans under circumstances which appeared to forbid the supposition of importation, a supposition moreover which seemingly a most thorough series of researches made at the time were thought definitively to set aside. The disease spread from New Orleans throughout the valleys of the Mississippi and its principal affluents, causing great mortality. In the work before us the history of this epidemic is given from point to point with the help of very numerous local contributors by Dr. Ely M'Clellan, of the United States Army; the history of the travels in Asia and Europe of the several epidemics which have reached America is given by Dr. John C. Peters; and the history of the several diffusions in North America by Dr. Ely M'Clellan. Dr. John J. Billings, U.S.A., contributes an almost exhaustive bibliography of cholera, and Dr. John M. Woodworth, the Supervising-Surgeon of the U.S. Mercantile Marine Hospital Service, a memorandum on the introduction of cholera through the agency of the mercantile marine. It is difficult to conceive the labour which must have been involved in getting together the materials collected by Dr. M'Clellan from the vast area covered by the epidemic, and from communities which do not possess any common system of death registration, and many of them no registration at all. His success is such as could hardly have been anticipated, and his report contains a mass of valuable information. Among this is an important report by Assistant-Surgeon Van Buren Hubbard, U.S.A., showing that the possibility of importation of cholera into New Orleans at the commencement of the outbreak had not been so certainly shut out by the early researches made as had been believed. Dr. Peters's report leaves the impression on the mind that many of his data require verifying by reference to the original sources. But the work generally, so far as it relates to the United States, is of the greatest value, and supplies much information before inaccessible.

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[1] Cholera Epidemic of 1873 in the United States. Reports prepared under the directions of Congress. Washington, Government Printing Office, 1875.

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